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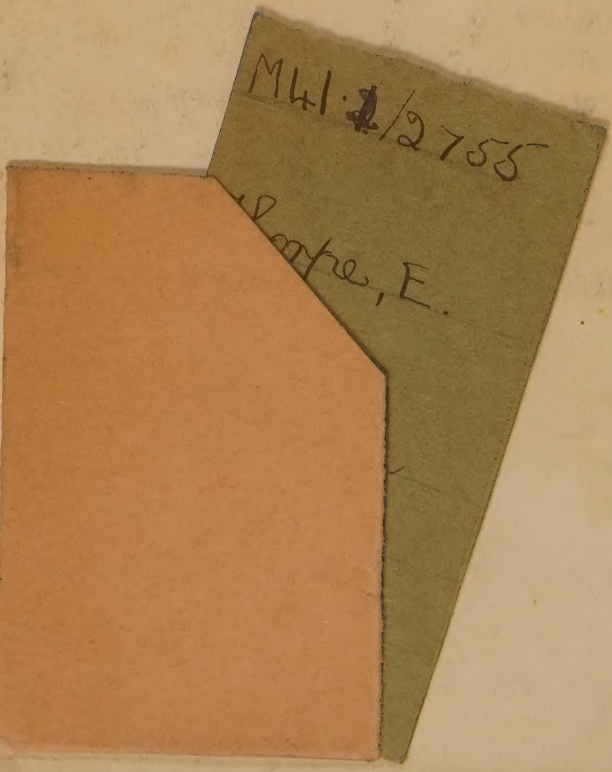

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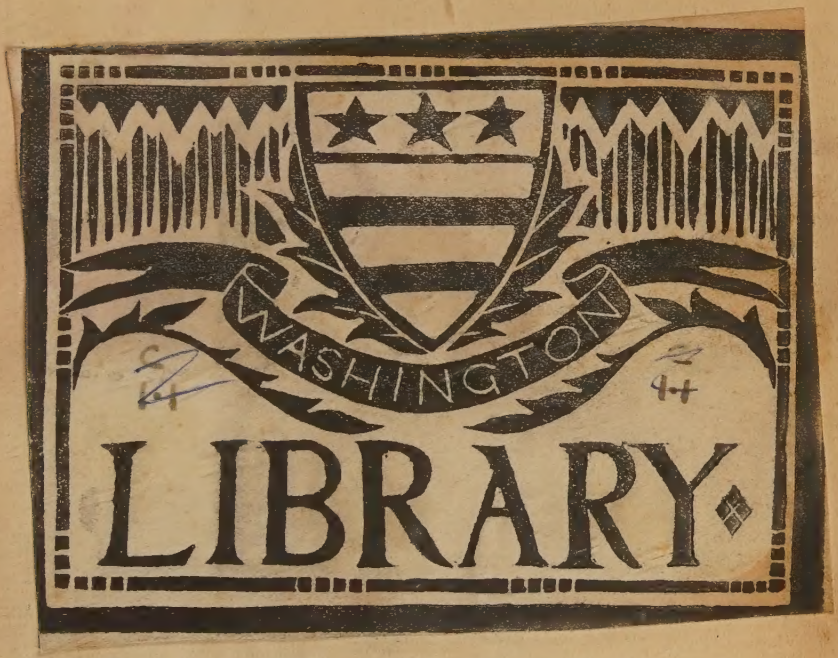

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ESSAYS IN HISTORICAL CHEMISTRY

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ESSAYS

IN

HISTORICAL CHEMISTRY

BY

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IMPERIAL COLLEGE OF SCIENCE AND TECHNOLOGY, LONDON

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PREFACE

This book consists mainly of lectures and addresses given at various times, and to audiences of very different type, during the last forty years. These essays in historical chemistry are now put together with the object of showing how the labours of some of the greatest masters of chemical science have contributed to its development. The book has no pretensions to be considered a history of chemistry, even of the time to which its narratives relate. Many honoured names—as Black, Dalton, Berzelius, Liebig, Hofmann—that ought, in all fitness, to find a fuller notice in such a series of biographical sketches, are only incidentally mentioned. The only excuse I can advance is, that it has not as yet been my good fortune to be in a position to offer an account of their labours.

The greater number of the sketches in the present volume have already been seen in print; but in arranging them for republication I have not hesitated to make such alterations and corrections as seemed necessary or desirable in view of their appearance in a connected series. Certain of the lectures, when delivered, were illustrated by experiments of which mention was made in the accounts originally published. It seemed useless to retain these references, and they have consequently

been omitted. The lectures, too, for an obvious reason, have been arranged in historical sequence, and not in the order in which they were written or delivered. Hence, in some cases, it happens that what now appear as successive chapters have in reality been composed at wide intervals of time, and addressed to audiences of very dissimilar character. Although, as stated, a certain amount of pruning has been done, there are occasional repetitions; possibly also a few inconsistent statements may be detected on comparing the earlier with the later essays—more, I trust, in matters of opinion than of fact. This is almost inevitable, unless some portions had been recast, or, to a greater or less extent, rewritten —which, as the essays are, to all intents and purposes, reprints, I did not feel justified in doing. It is to be expected that the wider knowledge which should follow upon many years of reading and study would modify, or possibly even altogether change, the impressions of the earlier time.

My thanks are due to the Proprietors and Editors of the *Contemporary* and *Fortnightly Reviews* and *Knowledge* for permission to include certain articles which have appeared in those periodicals. I am also indebted to the Council of the British Association for the Advancement of Science for permission to reprint the Presidential Address delivered to the Chemical Section of the Association at the meeting in Leeds in 1890. Messrs. Macmillan and the editor have allowed me to make use of certain articles contributed to *Nature*; the Managers of the Royal Institution have permitted me to reprint the lecture on Wöhler; the Council of the Chemical Society, those on Kopp, Victor Meyer, and Julius

Thomsen, and the Presidential Address of 1900; the Council of the Philosophical Society of Glasgow, the Graham Lecture, given in 1887; and the Council of the Greenock Philosophical Society, the Watt Anniversary Address of 1898. Lastly, I have to thank Mr. John Heywood, the publisher of the Manchester Science Lectures, for granting me permission to make use of the lectures on Priestley and Cavendish.

LONDON, *May* 1911.

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I

ROBERT BOYLE

One of the Free Evening Lectures delivered in connection with the Loan Collection of Scientific Apparatus at South Kensington in 1876.

From whatever point of view we may regard it, the period which began with the restoration of the House of Stuart and ended with its downfall is one of the most extraordinary in our history. It is a period of paradoxes. The reign of Charles II. is at once one of the worst and one of the brightest epochs in our annals. Never were the resources of this country so recklessly wasted; never was it more wretchedly governed. At home, public morality and political virtue were at their lowest ebb. Abroad, the foreign policy of the power which the firmness of Oliver had made to be everywhere respected was the subject of derision in even the smallest of German courts. The boys of Amsterdam, who, as Macaulay tells us, ran along the canals, when the great Protector was no more, shouting for joy that the Devil was dead, had as men the gratification of helping De Winter to burn our arsenals, and of insulting Tilbury, sacred to the memory of Elizabeth, and of one of the proudest moments of our national existence. On the other hand, at no former period were such mighty legislative reforms enacted; blow after blow was aimed at

and made its mark upon spiritual tyranny and territorial aggression; and the Church was made to admit, with Praed's good old Dr. Brown,

> That if a man's belief is bad,
> It will not be improved by burning.

If our literature was debased by the ribaldry of a crowd of dramatists and poetasters, it was purified and ennobled by the sublime genius of Milton and the brilliant fancy of Dryden.

Times so rich in incongruities, the times of Clarendon, Halifax, Hale, Russell, Milton, and Jeremy Taylor; and of Buckingham, Sunderland, Jeffreys, Oates, and the Duchess of Portsmouth, have been the wonder and the despair of historians. The inquiry how, under such untoward circumstances, such a marvellous progress was possible, was a riddle which it has been left to our own age to solve. This movement was the effect of that vague and indefinable force we call the spirit of the age; and the spirit of that age, as it has been laid bare to us by the searching anatomy of the author of the *History of Civilisation in England*, was a sceptical, inquiring, reforming spirit. It pervaded every department of knowledge and of intellectual energy. It was rife in theology, in politics, in philosophy, and eventually in science. This spirit may be said to have infused itself in science with the appearance in 1661 of a little octavo volume from an Oxford printing press: it came forth without any preparatory bustle, anonymously and undedicated. But in its revolt against mere authority, in its disdain of old-world notions, and in its ill-concealed contempt for the schoolmen, it so exactly caught and expressed the spirit of the time that it instantly arrested the attention of the learned world, and not only

of the small world of the *virtuosi*, but of that infinitely larger public of thinking men who felt a growing impatience of the dogmas of the schools. The book was entitled the "Sceptical Chymist: or Chemico-Physical Doubts and Paradoxes touching the Experiments, whereby vulgar Spagyrists are wont to endeavour to evince their Salt, Sulphur, and Mercury to be the true Principles of Things." There was not much in such a title to attract the common public, nor were its merits as a piece of literary workmanship of a very high order; nevertheless, the book was eagerly bought up, and its popularity was such as to attract even the attention of foreigners visiting London, and no fewer than ten Latin editions of it appeared on the Continent. Who was the author? was in everybody's mouth. Men declared that the mantle of the great lawgiver who, as Cowley sung, had seen, as from the summit of Pisgah, the land which he was not permitted to enter, had fallen upon him. The writer was soon identified as a young gentleman, the youngest son of an Irish peer, who the year before had ventured abroad a treatise on the elastic power of the air, in which he exploded the notion of a *Fuga Vacui*, and for doing which he had drawn down upon himself the trenchant wrath of the author of the *Leviathan*—Hobbes of Malmesbury, the last man of note in England who did battle for the Plenists.

This young man was called the Honourable Robert Boyle: he was the seventh son and the fourteenth child of the Great Earl of Cork, and was born at Lismore, in the county of Waterford, on 25th January 1626. His father, Richard Boyle, a younger son of the younger branch of a Hertfordshire family which could trace its ancestry back to the times of Edward the Confessor, despairing of employment at home, had

resolved to push his fortunes in Ireland, and at twenty-two years of age found himself in Dublin with no other worldly possessions than a taffety doublet and a pair of black velvet breeches laced, a new Milan fustian suit, two cloaks and competent linen, a couple of tokens, a trusty rapier and dagger, and twenty-seven pounds three shillings in ready money. From these inconsiderable beginnings he built, as his son relates with pride, so plentiful and so eminent a fortune that his prosperity found many admirers but few parallels. Of the mother of Robert Boyle we learn little beyond that she was the daughter of Sir Geoffrey Fenton, Principal Secretary of State for Ireland, that she wanted not beauty, and was rich in virtue. She died when her youngest son was only a few years old, and he tells us that he ever counted it among the chief misfortunes of his life that he knew not her that gave it him. When eight years of age he was sent to Eton, at which time Sir Henry Wotton was Provost: a fine gentleman himself, and well skilled in the art of making others so. Here the studious sickly boy with his uncouth manners, his stuttering speech, and his roving habits, must have been sorely tried had he not fallen into good hands: as it was, Eton and Sir Henry were ever pleasant memories to him. It might have been otherwise, however, for through a stupid mistake of a careless apothecary he had nearly lost his life; which accident, he said, made him long after apprehend more from the physicians than from the disease, and was possibly the occasion that made him so inquisitively apply himself to the study of physic that he might have the less need of them that profess it. Years after he himself was nominated to the Provostship by Charles II., but his objection to take orders, in spite of the advice of

Clarendon, overcame his inclination to accept an office to which his habits and associations disposed him. At the age of twelve he was sent with an elder brother to the Continent, where he remained for six years. The good old earl his father died in the midst of the troubles occasioned by the insurrection in Ireland, and Boyle with great difficulty found his way back to England and to his manor at Stalbridge, in Dorsetshire, which had descended to him. Here he lived in great retirement throughout the unhappy times which culminated in the death of Charles I., seeking in his books and in his laboratory some diversion from the contemplation of the miseries of his country. At about this time Boyle became a member of what its promoters pleasantly termed the Invisible College, an assembly of learned and curious gentlemen who applied themselves to the study of experimental science, or, as it was then called, the New Philosophy. The little band included John Wallis, the mathematician; John Wilkins, afterwards Bishop of Chester; Jonathan Goddard, Warden of Merton; Samuel Foster, Professor of Astronomy at Gresham College; and Theodore Haak, a German resident in London, who appears to have first suggested the meetings. These were held weekly at each other's lodgings in London or at Gresham College, "to discourse and consider of philosophical inquiries and such as related thereunto (precluding matters of theology and State affairs)." A certain portion of the company removed to Oxford, and continuing the meetings, were joined by Seth Ward, afterwards Bishop of Salisbury; Ralph Bathurst, President of Trinity College, Oxford; Dr., afterwards Sir William, Petty; Thomas Willis, and others. Boyle followed in 1654, and thereafter the philosophers met at his lodging, with Crosse, an

apothecary, for the convenience of inspecting drugs. The Oxford section, however, always seemed to regard their Gresham brethren as constituting the parent society, and from time to time they journeyed up to London for the purpose of attending the meetings at the College. Out of such beginnings grew the Royal Society of London for Improving Natural Knowledge, incorporated by Charles II. in 1663; and in the charter Boyle is named as one of the council. The growth of the new philosophy excited the jealousy and anger of those who affected to see in the ascendency of the Baconian method the subversion of everything that was orderly and of good repute. Religion, they cried, was being undermined; civil law was gone; the empire of reason and of all true learning was at an end. Bishops anathematised; Hobbes, who certainly had scant affection for the clergy, thundered; Butler lampooned. Boyle was earnestly solicited to leave the society. "I beseech you, sir," writes one of his correspondents, "consider the mischief it hath occasioned in this once flourishing kingdom, and if you have any sense, not only of the glory and religion, but even of the being of your native country, abandon that constitution. It is too much that you contribute to its advancement and repute: the only reparation you can make for that fatal error is to desert it betimes. Do not you apprehend that all the inconveniences that have befallen the land, all the debauchery of the gentry (which ariseth from that pious and prudent breeding, which was and ought to have been continued) will be charged on your account? . . . It will be impossible for you to preserve your esteem but by a seasonable relinquishing of these impertinents." The writer, Henry Stubbe, a physician of repute, was one of those unquiet spirits of

which the times were fertile: he was formerly a Student of Christ Church and Keeper in the Bodleian Library, where he wrote several tracts; his *Essay on the Good Old Times* was pardonable, but his *Apology for the Quakers* was too much for the patience of the University, and he lost his position in Oxford. At the time of the outcry against the Royal Society Stubbe made himself the champion of his faculty, the majority of whom condemned Sydenham for believing that the new-fangled philosophy and physic might have something good in common, and he fell foul of Glanvill, Rector of Bath, who had followed Sprat in lauding the institution and objects of the society, in his characteristic fashion. The controversy raged with no little fury and bitterness, and hard knocks were freely given and returned, as was the manner of the time. Fate decreed, however, that Glanvill should have the last word, for his adversary being accidentally drowned near Bath, it fell to the rector's lot to preach his funeral sermon. And let us hope that he saw in it, as the French say, a grand opportunity for holding his tongue.

But the Royal Society, notwithstanding the rough usage of its youth, continued to grow and prosper, and people began even to see that it might be of use to them in their day and generation. The Great Plague of 1665 and the Great Fire of 1666 gave the Society an opportunity, and much of what was good in the arrangement in the new city was the result of their deliberations and counsel. Science became even fashionable. The King set up a laboratory, and amused himself by making weather observations with the newly invented baroscope; the fine ladies of his court marvelled at the properties of the phosphorus (of so curious an origin) which Mr. Kraffts had brought from Germany; and the gentlemen

at Will's conversed of the *Vacuo Boyliano* and the spring of air. Moreover, many of the matters upon which the learned world at that time disputed were, when stated in intelligible terms, of common interest. One of the points about which people wrangled, when reduced to a plain issue, was this: Is a vacuum possible; that is, can a space absolutely void of matter be obtained? A few years before a very learned Frenchman, René Descartes, had asserted that, as according to his thinking the universe was absolutely full, it was impossible even to conceive of the existence of a vacuum; and such was his subtlety and logic that many other learned persons came to be of his opinion. But when men began to use their hands and eyes as well as their reason in attempting to get at nature's secrets, doubts arose whether the explanations and hypotheses of the gown-men were not rather strained, and for the most part unsatisfactory. For example, the manner in which Mr. Hobbes explained the action of the watering-pot would scarcely commend itelf to the readers of his *Dialogus Physicus de Natura Aeris*:—"If a gardener's watering-pot be filled with water, the hole at the top being stopped, the water will not flow out at any of the holes in the bottom; but if the finger be removed to let in the air above, it will run out at them all, and as soon as the finger is applied to it again the water will suddenly and totally be stayed again from running out. The cause whereof seems to be no other but this, that the water cannot, by its natural endeavour to descend, drive down the air below it, because there is no place for it [the air] to go into, unless either by thrusting away the next contiguous air it proceed by continual endeavour to the hole at the top, where it may enter and succeed in the place of the water that floweth out,

or else by resisting the endeavour of the water downwards penetrate the same, and pass up through it."

Unfortunately for the Plenists, as Mr. Hobbes and the Cartesians came to be called, there were some awkward facts which did not seem to agree at all with the notion of the plenitude of the world. There was the fact that if a tube, say 35 feet long, closed at one end and open at the other, be completely filled with water and inverted with the open end under water, the level of the water in the tube will sink a few feet—that is, the water-column will not exceed some 32 or 33 feet in length, measured from the level of the liquid in the cistern. Moreover, if the experiment be repeated with quicksilver, which is more than thirteen times heavier than water, bulk for bulk, the height of the quicksilver column will be only one-thirteenth of that of the water. And there was this particularly awkward fact, that if the tube containing the quicksilver be carried up a high tower, as did Claudio Bereguardi up the leaning tower of Pisa, or up a mountain, as did Perrier some four or five years later in France, or as Mr. Richard Townley did up one of the Lancashire hills, the space above the mercury became much greater as the summit was approached, and became less again as the descent was made. Curious persons naturally asked why the mercury behaved in this way, and what was in the space above the level of the liquid? If the world were actually as full as an egg, the existence of these apparently empty spaces was certainly very perplexing. It was not at all clear why nature should be so partial in her likings and dislikings as to put up with a much bigger space from the mercury than she would from the water, and it seemed rather irrational for her to hate a vacuum less at the top of a mountain than at the bottom.

It was about this time that Boyle published in the form of a letter to his nephew, Lord Dungarvan, his "New Experiments Physico-Mechanical touching the Spring of Air and its Effects, made for the most part in a new Pneumatical Engine." Shortly after Boyle had turned his attention to physical science he heard of a book "published by the industrious Jesuit Schottus, wherein it was related how that that ingenious gentleman, Otto Gericke, Consul of Magdeburg, had lately practised in Germany a way of emptying glass vessels by sucking out the air at the mouth of the vessel plunged under water." Boyle at once recognised that important results might be expected to follow the study of the phenomena of the air's rarefaction, but he also saw that such results could scarcely be furnished by Von Guericke's method. He accordingly sought to devise a more perfect form of instrument, and with the assistance of Robert Hooke, a man of remarkable inventive powers, he, about the year 1658, contrived his "Pneumatical Engine."

It consisted of a large pear-shaped vessel holding about thirty wine-quarts, fitted with a stopper at the top and connected at the bottom with a brass cylinder in which was a piston worked by a rack and pinion. Between the glass vessel, "which we," says Boyle, "with the glassmen shall often call a receiver for its affinity to the large vessels of that name used by chemists," and the cylinder was a stopcock which was alternately opened and closed as the piston was worked up and down, the air from the cylinder being allowed to escape through a small hole at the top, temporarily closed by a stopper. The mode of working the pump will be obvious. "By the repetition of the motion of the sucker [piston] upward and downward, and by

opportunely turning the key [of the stopcock] and stopping the valve [the brass peg inserted into the cylinder] as occasion requires, more or less air may be sucked out of the receiver according to the exigency of the experiment and the intention of him that makes it."

Before describing his experiments in detail Boyle proceeds to "insinuate that notion by which it seems likely that most if not all of them will prove explicable, namely, that there is a spring or elastical power in the air we live in. By which *elater* or spring of the air, that which I mean is this: that our air either consists of, or at least abounds with, parts of such a nature, that in case they be bent or compressed by the weight of the incumbent part of the atmosphere, or by any other body, they do endeavour, as much as in them lieth, to free themselves from that pressure, by bearing against the contiguous bodies that keep them bent; and, as soon as those bodies are removed, or reduced to give them way by presently unbending and stretching out themselves, either quite, or so far forth as the contiguous bodies that resist them will permit, and thereby expanding the whole parcel of air these elastical bodies compose." Boyle pictured to himself this process of unbending and stretching by considering the air near the earth to be "such a heap of little bodies lying one upon another as may be resembled to a fleece of wool. For this (to omit other likenesses betwixt them) consists of many slender and flexible hairs; each of which may indeed, like a little spring, be easily bent or rolled up; but will also, like a spring, be still endeavouring to stretch itself out again. For though both these hairs, and the aerial corpuscles to which we liken them, do easily yield to external pressures; yet each of them (by virtue of its structure) is endowed with a power or principle of self-

dilatation; by virtue whereof, though the hairs may, by a man's hand, be bent and crowded closer, and into a narrower room than suits best with the nature of the body; yet, whilst the compression lasts, there is in the fleece they compose an endeavour outwards, whereby it continually thrusts against the hand that opposes its expansion. And upon the removal of the external pressure, by opening the hand more or less, the compressed wool doth, as it were, spontaneously expand or display itself towards the recovery of its more loose and free condition, till the fleece hath either regained its former dimensions, or at least approached them as near as the compressing hand (perchance not quite opened) will permit."

This passage illustrates in a remarkable manner the mechanical turn of Boyle's mind, and the extreme caution with which he invariably expressed his opinions. Humboldt, indeed, calls him "the cautious and doubting Robert Boyle." He was well aware that other modes of explaining the elasticity of the air were possible, and, in fact, he cites that of Descartes, that the air is nothing but a heap of small flexible particles raised by the sun's heat "into that fluid and subtle and ethereal body which surrounds the earth; and by the restless agitation of that celestial matter, wherein those particles swim, are so whirled round that each corpuscle endeavours to beat off all others from coming within the little sphere requisite to its motion about its own centre; and in case any, by intruding into that sphere, shall oppose its free rotation to expel or drive it away." The vehement agitation which the particles receive from the fluid aether that swiftly flows between and whirls about each of them, as the eddying stream about the corks, not only keeps them separated, but also makes them hit against

and knock away each other, and consequently require more room than they would need if compressed. After all, there is a certain resemblance in this to our modern notions of the constitution of a gas. On the whole, Boyle is inclined to his own hypothesis, but he is unwilling, as he says, "to declare peremptorily for either of them against the other"; for "to determine whether the motion of restitution in bodies proceed from this, that the parts of a body of a peculiar structure are put into motion by the bending of the spring, or from the endeavour of some subtle ambient body whose passage may be stopped or obstructed, or else its pressure unequally resisted by reason of the new shape or magnitude, which the bending of a spring may give the pores of it: to determine this, I say, seems to me a matter of more difficulty than at first sight one would easily imagine it."

Boyle had a perfectly clear conception of the materiality of air, and he attempted on several occasions to determine its weight, although it is remarkable that he who was so familiar with the principle of Archimedes that a body weighed in a fluid loses of its weight an amount equal to that of the bulk of fluid displaced, should have made the experiment by weighing bladders first empty and then inflated with air. Indeed, he himself was the first to show that a bladder containing air and counterpoised with metallic weights appears to weigh more *in vacuo* than in the air; and the familiar experiment in which the cork ball seems to increase in weight when placed in the exhausted receiver was first devised by him.

"Taking it for granted then," he goes on to say, "that the air is not devoid of weight, it will not be uneasy to conceive that that part of the atmosphere wherein we live, being the lower part of it, the corpuscles that compose

it are very much compressed by the weight of all those of the like nature that are directly over them; that is, of all the particles of air that being piled up upon them, reach to the top of the atmosphere." He then recalls to mind the observation that the mercurial column in the barometer stands lower at the top of a mountain than at the bottom, "of which the reason seems manifestly enough to be this, that upon the tops of high mountains the air, which bears against the restagnant quicksilver, is less pressed by the less ponderous incumbent air." He next disposes of the possible objection that the air thus strongly compressed by the superincumbent atmosphere should yet yield readily to the motion even of little flies and feathers, by demonstrating "that it is the equal pressure of the air on all sides upon the bodies that are in it, which causeth the easy cession of its parts, which may be argued from hence; that if by the help of our engine the air be but in great part, though not totally, drawn away from one side of a body without being drawn away from the other, he that shall think to move that body to and fro, as easily as before, will find himself much mistaken." To demonstrate this Boyle was wont to tie a partially inflated bladder to the stopper of the receiver, and to desire a bystander to lift up the stopper after the receiver was partly exhausted: it was "pleasant to see," he says, "how men will marvel that so light a body, filled at most with but air, should so forcibly draw down their hands, as if it were filled with some very ponderous thing." The distension of the bladder, in consequence of the expansion of the included air, as the rarefaction in the receiver proceeded, affords him an additional proof of the force of the spring of air. He proceeds to point out that the force of this spring may be augmented by heat: "the elastical power

of the same quantity of air may be as well increased by the agitation of the aerial particles (whether only moving them more swiftly and scattering them, or also extending or stretching them out, I determine not) within an every way inclosing and yet yielding body; as displayed by the withdrawing of the air that pressed it without. For we found that a bladder but moderately filled with air and strongly tied, being awhile held near the fire, not only grew exceedingly turgid and hard, but afterwards being brought nearer to the fire suddenly broke with so loud and vehement a noise as stunned those that were by, and made us for a while after almost deaf." The connection of these phenomena singularly impressed Boyle; and he says that it deserves "deliberate speculation." During the two centuries which have elapsed since then many men have given this matter a vast amount of "deliberate speculation," with the result of showing that this connection is even more intimate than Boyle, with all his prevision, could have dreamt of.

The relation of the air to combustion, and the nature of flame, attracted much attention from Boyle, and he frequently returned to these subjects in the course of his work. His observations on the burning of candles in a partial vacuum are worth mention for the evidence they afford of the care with which he noted even the minutest phenomena attending an experiment. After proving that the flame is extinguished long before the exhaustion is complete, he goes on to say "that these things were further observable, that after the two or three first exsuctions of the air, the flame (except at the very top) appeared exceedingly blue, and receded more and more from the tallow, till at length it appeared to possess only the very top of the wick, and there it went out." These phenomena, apparently so trivial, are now recog-

nised as of importance in connection with the theory of illuminating flames.

Boyle next proceeds to what he evidently regards as a great *experimentum crucis*, "whereof," he says, "the satisfactory trial was the principal fruit I promised myself from our engine": it related to the behaviour of a barometer in the exhausted receiver. After carefully fitting the barometer into the receiver, so that the outer air could not press down upon the surface of the metal in the cistern, he drew down the sucker, and found to his delight that the mercury fell within the tube and continued to fall so long as the pump was worked until it was only an inch or so from the level of that in the cistern: on readmitting the air the mercury was impelled up again to its original position in the tube. The importance of this observation was obvious, and all Oxford came to see an experiment which afforded such a signal confirmation of the truth of the speculations of Galileo and Pascal. It now occurred to Boyle to try what relation existed between the height of the mercurial column and the number of suctions made by the pump, for he had observed that the first sucks caused a far more rapid decrease in the height than the last. Boyle, we see, is now on the verge of the great discovery which has made his name familiar to every schoolboy in this country. It is worth noticing that it was in all probability the accident of the mode of construction of his engine, and the fact that each suction drew out a determinate bulk of air, that induced him to attempt to determine the relation between the pressure and volume of the air. He was forced, however, to abandon the attempt at this time, for he found that with the apparatus in its present form he was unable to make observations accurate enough to reduce them to

any hypothesis. "Yet," he adds, "would we not discourage any from attempting it, since if it could be reduced to a certainty it is probable that the discovery would not be unuseful."

He is now forced to confront the ever-recurring question — Is there a vacuum? and accordingly he proceeds to take the arguments of the Plenists to pieces. What proof, he asks, do they offer of the existence of that subtle ethereal matter which they say must exist in the space above the mercury. Why must exist? Because, they answer, there cannot be a void. And there cannot be a void because extension is the only nature of a body, and to say a space is devoid of body is, to the schoolmen, a contradiction *in adjecto*. The matter is, in fact, reduced to a question of metaphysics, and Boyle gives it up, "finding it very difficult either to satisfy naturalists with this Cartesian notion of a body, or to manifest wherein it is erroneous." The truth is, Boyle was hampered by his corpuscular notions, or he would assuredly have gone over to the Vacuists. He puts his candles and his bladders into his receivers, and however completely he may pump out the air, the things are none the less visible, and he asks—Can it be seriously imagined that light can be conveyed from an object without some vehicle to convey it?

He then substituted water for mercury, and repeated the experiment. As the rarefaction proceeded, he was struck with the appearance of a multitude of air bubbles within the liquid. The origin of this air puzzled him greatly. Was the water turned into air, or was the air pre-existent and latitant in the water? On the whole, he inclines to the latter supposition, but mainly for the reason that all experience showed that water was elementary, indestructible, and inconvertible. He argues

the matter at such length that he is constrained to apologise for his prolixity in treating of such empty things as bubbles; yet he does not fail to point the moral "that there are very many things in nature that we disdainingly overlook as obvious or despicable, each of which would exercise our understandings, if not pose them too, if we would but attentively enough consider it, and not superficially contemplate it, but attempt satisfactorily to explicate the nature of it."

The idea that the air was the medium by which sound is ordinarily conveyed was familiar enough to the philosophers of the seventeenth century, and Boyle furnishes a proof of the fact by the observation that the ticking of a watch placed in the receiver became inaudible when the air was withdrawn.

The mode of action of the syphon next engages his attention, and he proceeds to inquire what must be the height of the atmosphere on the assumption that it has the same density at all points that it possesses on the earth's surface. He has completed the proof that the pressure of the air supported the mercurial column; his problem was to determine how much heavier mercury is than air, bulk for bulk; he would thus be able to calculate the height of a column of air, of the density of that on the earth's surface, required to balance a mercurial column of equal base and of 30 inches in height. Boyle unfortunately considered that the ratio of the weights of equal bulks of water and air was known with sufficient accuracy in his day, and after a discussion of all the observations with which he was acquainted, he concludes that water may be considered to be 1000 times heavier than air, which we now know to be greatly in excess of the truth. He proceeds, then, to inquire how much heavier mercury is than water, but

the observations of his predecessors on this point are so discordant that he feels himself obliged to re-determine the relation, firstly by observing the heights of counter-balancing columns of mercury and water in a U-shaped tube, and, secondly, by the method now adopted as the most accurate of all modes of estimating the specific gravities of liquids. By the first method he found that mercury is 13·7, by the second 13·68 times heavier than water: no very great disparity from the number 13·6 which we now adopt. From these data Boyle calculated that the atmosphere must be between six and seven miles high, on the supposition that it has the same density throughout that it has on the surface of the earth: in reality, on the same assumption, it is only between five and six miles high. Boyle was perfectly aware that this result was, in a sense, fictitious, but he shows that it was not without value as demonstrating what must be the minimum height of the atmosphere: it proved that the conjectures of Kepler and others that the air could not extend beyond a couple of miles or so from the earth's surface were certainly erroneous.

The main fact that air is related to life was of course as well understood in those days as it is now, but very little was known of the theory of respiration. Boyle made many experiments with his air-engine to elucidate this matter, and I am really afraid, in these anti-vivisection days, to tell you how many cats, mice, sparrows, fishes, tadpoles, and snails fell victims to his zeal. Not that he inflicted needless suffering, for Boyle was the most tender-hearted of men; if he has occasion to confine a mouse all night in one of his receivers, he places him near the fire, and consoles him with a bit of cheese, that he may be as comfortable as circumstances will permit; a lusty and pugnacious sparrow makes such a resolute

stand for existence that Boyle is fain to let him go; and the intercession of a lady is quite sufficient to deprive a certain kitten of the honour and glory of settling an important query concerning respiration.

The last experiment that Boyle describes is one of the most important and striking in the whole series, since by means of it he demonstrated how dependent is the boiling-point of a liquid upon the atmospheric pressure. Having boiled some water "a pretty while that by the heat it might be freed from the latitant air," he placed it, whilst still hot, within the receiver, when, on exhaustion, it again began to boil "as if it had stood over a very quick fire. . . . Once, when the air had been drawn out, the liquor did, upon a single exsuction, boil so long with prodigiously vast bubbles that the effervescence lasted almost as long as was requisite for the rehearsing of a *Pater Noster*. This experiment," he says, "seems to teach that the air by its stronger or weaker pressure may very much modify (as the schoolmen speak) divers of the operations of that vehement and tumultuous agitation of the small parts of bodies, wherein the nature of heat seems chiefly, if not solely, to consist."

Such is a very rapid and a very imperfect summary of this great work. I have purposely quoted very largely from it, for I wished to show you, in Boyle's own words, how wonderfully near much of the philosophy of the seventeenth century is to that which we are too apt to regard as the outcome of the nineteenth. It is impossible to exaggerate the importance of Boyle's labours; they served to give a marvellous sharpness to the notions of that time concerning the materiality of the air and of the phenomena which depend upon its elasticity. The work exhibits in an eminent degree Boyle's character as

an investigator, his quick perception and receptive mind, his great power of co-ordination, his insight, his logic, his patient care and scrupulous accuracy. It exhibits, too, his weakness; for it must be admitted that it is wanting in that grasp of principle and faculty of generalisation which we see in the work of the illustrious author of the *Novum Organum*. It lacks, too, the *Forscherblick* and power of divination so characteristic of the genius of Newton. But to say that Boyle is only inferior to Bacon and Newton is to assign him one of the first niches in the Walhalla of the heroes of science.

But Boyle's work, as I have before hinted, was not allowed to go forth unchallenged; and the Elaterists were quickly taken to task, on the one hand by one Franciscus Linus, and on the other by a far more important personage—Thomas Hobbes, of Malmesbury. Hobbes has been styled the subtlest dialectician of his time, and a master of precise and luminous language; too frequently, however, that language lost more in elegance than it gained in force. Hobbes, although not a professed Peripatetic or a Cartesian, was a very pronounced Plenist. He utterly failed to see any virtue in the new philosophy, and the disparagement of the Gresham set, or "the experimentarian philosophers," as he sneeringly called them, was the chief design of his *Dialogus Physicus de Natura Aeris*, the book in which he attempts to write down Boyle and his work. Boyle hated contention; but he and his friends felt that the new doctrines were at stake. It is unnecessary for me to take up your time by examining Mr. Hobbes's arguments or Boyle's refutation of them; it is sufficient to say that Mr. Hobbes, who had, with singular indiscretion, laid himself open by quoting Vespasian's law, "*That it is unlawful to give ill language first, but civil and*

lawful to return it," was taught politeness and much sound philosophy. The world will willingly let the *Dialogus* die, or remember it only in connection with Boyle's *Examen* of it.

We cannot, however, so summarily dismiss Franciscus Linus. Linus sets out to prove that the mercury in the Torricellian experiment is upheld not by the pressure of the air but by a certain nondescript internal cord; and Boyle undertakes to show that this hypothesis of an internal *funiculus*, which he remarks, with quiet humour, "seems to some more difficult to conceive than any of the phenomena in controversy is to be explained without it, is 'partly precarious, partly unintelligible, partly insufficient, and besides needless.'" Indeed the matter is scarcely worth mention except for the circumstance that it gave an occasion to Boyle to return to the question, which we have seen had already interested him, of the relation between the volume and the pressure of the air. In the answer to Linus he gives two new experiments touching the measure of the force of the spring of air compressed and dilated. The account of these memorable experiments must be given in Boyle's own words: "We took then a long glass tube, which, by a dexterous hand and the help of a lamp, was in such a manner crooked at the bottom, that the part turned up was almos parallel to the rest of the tube, and the orifice of this shorter leg of the syphon (if I may so call the whole instrument) being hermetically sealed, the length of it was divided into inches (each of which was subdivided into eight parts) by a straight list of paper, which, containing those divisions, was carefully pasted all along it. Then putting in as much quicksilver as served to fill the arch or bended part of the syphon, that the mercury standing in a level might reach in the

one leg to the bottom of the divided paper, and just to the same height or horizontal line in the other, we took care, by frequently inclining the tube, so that the air might freely pass from one leg into the other by the sides of the mercury (we took, I say, care), that the air at last included in the shorter cylinder should be of the same laxity with the rest of the air about it. This done, we began to pour quicksilver into the longer leg of the syphon, which, by its weight pressing up that in the shorter leg, did by degrees straighten the included air; and continuing this pouring in of quicksilver till the air in the shorter leg was by condensation reduced to take up but half the space it possessed (I say possessed, not filled) before, we cast our eyes upon the longer leg of the glass, upon which we likewise pasted a list of paper carefully divided into inches and parts, and we observed, not without delight and satisfaction, that the quicksilver in that longer part of the tube was 29 inches higher than the other. Now that this observation does both very well agree with and confirm our hypothesis, will be easily discerned by him that takes notice what we teach: and Monsieur Pascal and our English friend's [Mr. Townley's] experiments prove, that the greater the weight is that leans upon the air, the more forcible is its endeavour of dilatation, and consequently its power of resistance (as other springs are stronger when bent by greater weights). For this being considered, it will appear to agree rarely well with the hypothesis, that as according to it the air in that degree of density, and correspondent measure of resistance, to which the weight of the incumbent atmosphere had brought it, was unable to counterbalance and resist the pressure of a mercurial cylinder of about 29 inches, as we are taught by the Torricellian experiment; so here the same air being

brought to a degree of density about twice as great as that it had before, obtains a spring twice as strong as formerly. As may appear by its being able to sustain or resist a cylinder of 29 inches in the longer tube, together with the weight of the atmospherical cylinder that leaned upon those 29 inches of mercury; and, as we just now inferred from the Torricellian experiment, was equivalent to them."

At this stage of the experiments the tube broke, and it was only after several mischances that Boyle was able to complete his observations.

He then proceeded to the converse experiment—that is, to determine the spring of rarefied air. A tube, about 6 feet in length, and sealed at one end, was nearly filled with mercury, and into it was placed "a slender glass pipe of about the bigness of a swan's quill, and open at both ends; all along of which was pasted a narrow list of paper, divided into inches and half-quarters. This slender pipe being thrust down into the greater tube almost filled with quicksilver, the glass helped to make it swell to the top of the tube; and the quicksilver getting in at the lower orifice of the pipe filled it up till the mercury included in that was near about a level with the surface of the surrounding mercury in the tube. There being, as near as we could guess, little more than an inch of the slender pipe left above the surface of the restagnant mercury, and consequently unfilled therewith, the prominent orifice was carefully closed with sealing-wax melted; after which the pipe was let alone for a while that the air, dilated a little by the heat of the wax, might, upon refrigeration, be reduced to its wonted density. And then we observed, by the help of the above-mentioned list of paper, whether we had not included somewhat more or

somewhat less than an inch of air; and in either case we were fain to rectify the error by a small hole made (with a heated pin) in the wax, and afterwards closed up again. Having thus included a just inch of air, we lifted up the slender pipe by degrees, till the air was dilated to an inch, an inch and a half, two inches, etc., and observed in inches and eighths the length of the mercurial cylinder, which, at each degree of the air's expansion, was impelled above the surface of the restagnant mercury in the tube. The observations being ended, we presently made the Torricellian experiment with the above-mentioned great tube of 6 feet long, that we might know the height of the mercurial cylinder for that particular day and hour, which height we found to be 29¾ inches."

Such were the experiments, simple and easily made, which led Boyle to the recognition of the great law which bears his name—a law which is so far from being "unuseful" that it is recognised by the physicist as of the first importance. And yet in spite of the thoroughness with which Boyle did the work, and in spite, too, of the precision with which he stated his results, the attempt has not been wanting to deprive him of the whole merit of this discovery, and there is scarcely a text-book of physics or chemistry on the Continent, or at least in France, in which his name is mentioned in connection with the matter: abroad they prefer to ascribe the glory to the Abbé Mariotte, although Mariotte's treatise, *De la Nature de l'Air*, in which he enunciates the law, was not printed until seventeen years after Boyle had published his reply to Linus.

"The results of the two series of experiments here detailed are given in the following tables:—

A TABLE OF THE CONDENSATION OF THE AIR

A	A	B	C	D	E
48	12	00		$29\frac{2}{16}$	$29\frac{2}{16}$
46	$11\frac{1}{2}$	$01\frac{7}{16}$		$30\frac{9}{16}$	$30\frac{6}{16}$
44	11	$02\frac{13}{16}$		$31\frac{15}{16}$	$31\frac{12}{16}$
42	$10\frac{1}{2}$	$04\frac{6}{16}$		$33\frac{8}{16}$	$33\frac{1}{7}$
40	10	$06\frac{3}{16}$		$35\frac{5}{16}$	35
38	$9\frac{1}{2}$	$07\frac{14}{16}$		37	$36\frac{15}{19}$
36	9	$10\frac{2}{16}$		$39\frac{5}{16}$	$38\frac{7}{8}$
34	$8\frac{1}{2}$	$12\frac{8}{16}$		$41\frac{10}{16}$	$41\frac{2}{17}$
32	8	$15\frac{1}{16}$		$44\frac{3}{16}$	$43\frac{11}{16}$
30	$7\frac{1}{2}$	$17\frac{15}{16}$		$47\frac{1}{16}$	$46\frac{3}{5}$
28	7	$21\frac{3}{16}$		$50\frac{5}{16}$	50
26	$6\frac{1}{2}$	$25\frac{3}{16}$		$54\frac{5}{16}$	$53\frac{10}{13}$
24	6	$29\frac{11}{16}$		$58\frac{13}{16}$	$58\frac{2}{3}$
23	$5\frac{3}{4}$	$32\frac{3}{16}$		$61\frac{5}{16}$	$60\frac{18}{23}$
22	$5\frac{1}{2}$	$34\frac{15}{16}$		$64\frac{1}{16}$	$63\frac{6}{11}$
21	$5\frac{1}{4}$	$37\frac{15}{16}$		$67\frac{1}{16}$	$66\frac{4}{7}$
20	5	$41\frac{9}{16}$		$70\frac{11}{16}$	70
19	$4\frac{3}{4}$	45		$74\frac{2}{16}$	$73\frac{11}{19}$
18	$4\frac{1}{2}$	$48\frac{12}{16}$		$77\frac{14}{16}$	$77\frac{2}{3}$
17	$4\frac{1}{4}$	$53\frac{11}{16}$		$82\frac{12}{16}$	$82\frac{4}{17}$
16	4	$58\frac{2}{16}$		$87\frac{14}{16}$	$87\frac{3}{8}$
15	$3\frac{3}{4}$	$63\frac{15}{16}$		$93\frac{1}{16}$	$93\frac{1}{5}$
14	$3\frac{1}{2}$	$71\frac{5}{16}$		$100\frac{7}{16}$	$99\frac{6}{7}$
13	$3\frac{1}{4}$	$78\frac{11}{16}$		$107\frac{13}{16}$	$107\frac{7}{13}$
12	3	$88\frac{7}{16}$		$117\frac{9}{16}$	$116\frac{4}{8}$

Added to $29\frac{2}{16}$ makes

AA The number of equal spaces in the shorter leg that contain the same parcel of air diversely extended.

B The height of the mercurial cylinder in the longer leg that compressed the air into those dimensions.

C The height of the mercurial cylinder that counterbalanced the pressure of the atmosphere.

D The aggregate of the two last columns, **B** and **C**, exhibiting the pressure sustained by the included air.

E What that pressure should be according to the hypothesis that supposes the pressure and expansion to be in reciprocal proportion.

A TABLE OF THE RAREFACTION OF THE AIR

A	B	C	D	E
1	00	Subtracted from $29\frac{3}{4}$ leaves	$29\frac{3}{4}$	$29\frac{3}{4}$
$1\frac{1}{2}$	$10\frac{5}{8}$		$19\frac{1}{8}$	$19\frac{5}{8}$
2	$15\frac{3}{8}$		$14\frac{3}{8}$	$14\frac{7}{8}$
3	$20\frac{2}{8}$		$9\frac{4}{8}$	$9\frac{15}{12}$
4	$22\frac{5}{8}$		$7\frac{1}{8}$	$7\frac{7}{16}$
5	$24\frac{1}{8}$		$5\frac{5}{8}$	$5\frac{9}{20}$
6	$24\frac{7}{8}$		$4\frac{7}{8}$	$4\frac{27}{26}$
7	$25\frac{4}{8}$		$4\frac{2}{8}$	$4\frac{1}{4}$
8	$26\frac{0}{0}$		$3\frac{6}{8}$	$3\frac{23}{32}$
9	$26\frac{3}{8}$		$3\frac{2}{8}$	$3\frac{11}{86}$
10	$26\frac{6}{8}$		$3\frac{0}{0}$	$2\frac{39}{40}$
12	$27\frac{1}{8}$		$2\frac{5}{8}$	$2\frac{23}{48}$
14	$27\frac{4}{8}$		$2\frac{2}{8}$	$2\frac{1}{8}$
16	$27\frac{6}{8}$		$2\frac{0}{0}$	$1\frac{55}{64}$
18	$27\frac{7}{8}$		$1\frac{7}{8}$	$1\frac{47}{72}$
20	$28\frac{0}{0}$		$1\frac{6}{8}$	$1\frac{9}{80}$
24	$28\frac{2}{8}$		$1\frac{4}{8}$	$1\frac{23}{96}$
28	$28\frac{3}{8}$		$1\frac{3}{8}$	$1\frac{1}{16}$
32	$28\frac{4}{8}$		$1\frac{1}{8}$	$0\frac{111}{128}$

A The number of equal spaces at the top of the tube that contained the same parcel of air.

B The height of the mercurial cylinder that, together with the spring of the included air, counterbalanced the pressure of the atmosphere.

C The pressure of the atmosphere.

D The complement of **B** to **C** exhibiting the pressure sustained by the included air.

E What that pressure should be according to the hypothesis.

It would be quite impossible for me, in the time which remains, to attempt to go over, however superficially, the whole ground of Boyle's work, although there is much in it of special interest at the present time, as, for example, his papers on the *Saltness of the Sea, and the Nature of the Sea's Bottom*; and his *Essay of the Intestine Motions of the Particles of Quiescent Solids wherein the absolute Rest of Bodies is called in question.* He was perhaps the earliest to

draw attention to the desirability of studying the forms of crystals, and his paper on the *Figures of Salts* contains many curious observations; in his *Experiments about the Superficial Figures of Fluids, especially of Liquors contiguous to other Liquors*, he breaks ground which has taxed the energies of our greatest mathematicians. His *Treatise on Cold* abounds with striking and original experiments: thus he demonstrates the expansive power of freezing water by bursting a gun-barrel filled with water and securely plugged, by placing it in a mixture of snow and salt, a freezing mixture which he himself brought into use in England. His *Essays on the Usefulness of Experimental Natural Philosophy* were of the greatest service in his time in furthering the cause of science by showing how the material interests of civilisation may be promoted by its study; and, lastly, his tract on *Unsucceeding Experiments* must have been as the wine of gladness and the oil of consolation to many a despondent virtuoso. His fame and his social position made Boyle's personal influence very considerable, and his house (or rather that of his sister, with whom he lived, for he was never married) was constantly besieged by a crowd of patentees and inventors, who sought his aid in bringing their schemes to the notice of the Government or the King: he was thus the means of introducing into the marine a method of obtaining fresh water from sea-water, not very dissimilar to that which we owe to the late Dr. Normandy: this method, I need scarcely add, is not that of the ingenious youth who (whisper it not in the shades of Burlington Gardens!) gravely proposed to obtain fresh water from salt water by letting it stand and skimming it!

Boyle was a religious man, in the best sense of that

term, and his theological writings form no inconsiderable portion of his works. But we fear that Carneades and Eleutherius have made more stir, and, possibly, have done not less good in the world, than Lindamor and Eusebius. The *Christian Virtuoso* and the *Seraphic Love*, and possibly Swift's merciless *Pious Meditation on a Broomstick in the style of the Honourable Mr. Boyle*, have done more to perpetuate the *Occasional Reflections* than the *Occasional Reflections* have done for themselves.

Boyle was born in the year in which Bacon died: and Boyle's place in the history of science is that of the first true exponent of the Baconian method, and the *Sceptical Chymist* is his greatest work. This book probably contains a greater number of well-authenticated facts than is to be found in any other chemical treatise of its day. Many of these originated with Boyle, as, for example, the isolation of methyl alcohol from the products of the destructive distillation of wood, and that of acetone, which he prepared by heating the acetates of lead and lime.

But the greater merit of this work consists in its determined attack on the authority of the Peripatetics and the Paracelsians. Not that he is blind to the services of the Spagyrists: "the devisers and embracers of the hypothesis of the *tria prima* have done the commonwealth of learning some service by helping to destroy that excessive esteem or rather veneration, wherewith the doctrine of the four elements was almost as generally as undeservedly understood! The Peripatetics, thinking it more high and philosophical to discover truth *a priori* than *a posteriori*, scorn the experimental method as descending to the capacities of such as can only be taught by their senses: the

dialectical subtleties of the schoolmen much more declare the wit of him that uses them than increase the knowledge or remove the doubts of sober lovers of truth." Boyle is very severe upon the affected mysticism of the Spagyrists. They may be as obscure as they like about their elixir, and the rest of their grand arcana, "yet when they pretend to teach the general principles of natural philosophers, this equivocal way of writing is not to be endured. For in such speculative inquiries where the naked knowledge of the truth is the thing principally aimed at, what does he teach me worth thanks, that does not, if he can, make his notion intelligible to me, but by mystical terms and ambiguous phrases darkens what he should clear up, and makes me add the trouble of guessing at the sense of what he equivocally expresses, to that of learning the truth of what he seems to deliver." Boyle indeed does not scruple to say that the reason why the Spagyrists wrote so obscurely of their three great principles was, that not having clear and distinct notions of them themselves, they could not write otherwise than confusedly of what they had confusedly apprehended: they could scarcely keep themselves from being confuted but by keeping themselves from being clearly understood—home-thrusts which must have made many a Helmontian wince. The effect of such hard hitting is made evident on the most superficial comparison of the general style of chemical treatises immediately preceding Boyle's time with those published towards the close of the seventeenth century.

The *Sceptical Chymist* sealed the fate of the doctrine of the *tria prima*, and before the close of the century the Paracelsians were as much out of date as a Phlogistian would be to-day. Boyle indeed seems to incline

to the belief that all matter is compounded of one primordial substance—in other words, that all matters are merely modifications of the *materia prima*—and how closely he was in accord with the modern spirit is manifest in this remarkable passage: "I am apt to think that men will never be able to explain the phenomena of nature, while they endeavour to deduce them only from the presence and proportions of such or such material ingredients, and consider such ingredients or elements as bodies in a state of rest; whereas indeed the greatest part of the affections of matter, and consequently of the phenomena of nature, seem to depend upon the motion and contrivance of the small parts of bodies."

II

JOSEPH PRIESTLEY

A Lecture delivered in the Hulme Town Hall, Manchester, on 18th November 1874. Manchester Science Lectures.

Those of you who read newspapers will, probably, not have forgotten that on the 1st of August of this present year (1874) a great gathering took place at Birmingham to do honour to Joseph Priestley, one of that band of scientific worthies which made the reign of George III. memorable in the annals of science. On that day Professor Huxley (than whom no one is better qualified to appreciate the whole outcome of Priestley's life, or better able to set forth the singular force and beauty of his character) uncovered a statue which the friends of science and of liberal thought had raised to the memory of the philosopher. Birmingham, however, was not the only town in England, nor were Englishmen the only people, that did homage to the memory of Priestley on that day. The lovers of science in Leeds, near to which place he was born, assembled in public meeting; and the chemists of America, to which country he was driven by the political and theological bigotry of his own people, met together at his grave in a quiet little town on the banks of the Susquehanna river.

My object this evening is to give you some account of the labours of this philosopher, whose services in the

cause of truth, and whose sacrifices in the struggle for freedom of thought, were, seventy years after his death, thus gratefully recognised.

But the very richness of my material is a source of embarrassment; for Priestley was a man of so many and such diverse acquirements—

> A man so various, that he seemed to be
> Not one, but all mankind's epitome;

his energy and power of application were so great, the range of his work so wide, that to attempt to do full justice to the many-sidedness of the man and of his labours would require me to inflict on you, not one lecture alone, but a whole series. You may form some conception of his marvellous mental activity, when I tell you that, as appears from the catalogue drawn up by his son after his death, he published no fewer than 108 works. Among them we have two volumes *On the History and Present State of Discoveries relating to Vision, Light, and Colours*; next, two volumes of *Disquisitions relating to Matter and Spirit*; *A Course of Lectures on Oratory and Criticism*; *A General History of the Christian Church*, in six volumes; *The Doctrine of Phlogiston Established*; *A Treatise on Civil Government*; six volumes of *Experiments on Different Kinds of Air*; *A Harmony of the Evangelists in Greek*; *A Familiar Introduction to the Theory and Practice of Perspective*; and *The Rudiments of English Grammar, Adapted to the Use of Schools.* And this formidable development of the *cacoëthes scribendi* came, as he tells us, by a practice of abstracting sermons and writing much in verse.

Some particulars of the life of this extraordinary man may be interesting to you. He was born in 1733,

at Fieldhead, a hamlet of some half-dozen houses, about six miles from Leeds. The old home of the Priestleys was pulled down some years ago. It was described by one who pointed out its site to me, and who remembered it well, as a little house of three small rooms, built of stones and slated with flags. Jonas Priestley, the father, was a cloth-dresser by trade. Of the mother but little is known beyond that she was the daughter of a farmer living near Wakefield. She died when Priestley was only seven years old, and he was taken charge of by his aunt, a Mrs. Keighley, a pious and excellent woman, in a good position, but who, as he tells us, "knew no other use of wealth, or of talents of any kind, than to do good." The boy was of a weakly consumptive habit, one consequence of which was seen in the desultory character of his early education. But his home-life with his aunt must have done much to make up for the deficiencies of his school training. She encouraged him in his fondness for books, and as her house was the resort of all the dissenting clergymen in the district without distinction, young Priestley was constantly brought in contact with men of culture and of liberal thought, and several of them seem to have made a lasting impression on his vigorous mind. Still, the gloomy Calvinism under which he was brought up, and the frequent talk of *experiences* and of *new births* to which he listened, had its effect upon the sensitive mind in the weakly frame. Years afterwards he wrote of this period: "I felt occasionally such distress of mind as it is not in my power to describe, and which I still look back upon with horror. Notwithstanding I had nothing very material to reproach myself with, I often concluded that God had forsaken me, and that mine was like the case of Francis Spira, to whom, as he imagined,

repentance and salvation were denied. In that state of mind I remember reading the account of the man in the iron cage in *The Pilgrim's Progress* with the greatest perturbation." But the strengthening intellect was not slow to recover its ascendency; and Priestley could afterwards write, in his characteristic way of always looking at the sunny side of every circumstance: "I even think it an advantage to me, and am truly thankful for it, that my health received the check that it did when I was young; since a muscular habit from high health, and strong spirits, are not, I think, in general accompanied with that sensibility of mind which is both favourable to piety and to speculative pursuits."

Priestley was destined by his aunt for the ministry, but her views—which were his also—were for a time interfered with by his continued ill-health. Eventually he was sent to the Dissenting Academy at Daventry, which the labours of the good and learned Dr. Doddridge had brought into repute. Of the three years he spent there Priestley ever spoke with peculiar satisfaction. The system of study was congenial to his independent and inquisitive mind, for the freest inquiry on every article of theological orthodoxy and heresy was warmly encouraged, and every vexed question was in turn handled by the teachers, who took opposite sides in controversy, and incited their students to discussion. If training such as this laid the foundation of the successes of Priestley's after-life, it was also, and in no less degree, the source of much of his misfortune. His first charge, on leaving Daventry, was at Needham Market, in Suffolk; but his congregation did not like his Arianism, nor the stuttering way in which he told them of it, and they almost deserted him. Driven to extremities, he issued proposals to teach the classics and

mathematics for half a guinea a quarter, and to board the pupils in his house for twelve guineas a year. This scheme not answering, he next turned his attention to popular science, and commenced with a course of twelve lectures on "The Use of the Globes," from which he barely got enough to pay for his globes. Although he keenly felt the effects of what he terms his "low despised situation," Priestley never lost heart or hope. He could even say of his impediment in speech, that, like St. Paul's "thorn in the flesh," it was not without its use. "Without some such check as this," he writes, "I might have been disputatious in company, or might have been seduced by the love of popular applause as a preacher; whereas my conversation and my delivery having nothing in them that was generally striking, I hope I have been more attentive to qualifications of a superior kind."

Years afterwards, on being invited to preach in the district when he had raised himself to some degree of notice in the world, the same people crowded to hear him; and though his elocution was not much improved, they professed to admire one of the same discourses they had formerly despised.

From Needham he passed on to Nantwich, in Cheshire, where he found himself in more congenial society, and in better circumstances, so that he was able to buy books and a few philosophical instruments. Not that philosophy here occupied the whole of his leisure, for he tells us that he betook himself to music, and learned to play on the English flute, as the easiest instrument. Music he recommends to all studious persons; and it will be better for them, he says, if, like himself, they should have no very fine ear or exquisite taste, as by this means they will be more easily pleased,

and be less apt to be offended when the performances they hear are but indifferent. In 1761 he was invited to Warrington as "tutor in the languages" in the Dissenting Academy in that town. Here he taught Latin, Greek, Hebrew, French, and Italian; and delivered courses of lectures on Logic, on Elocution, on the Theory of Language, on Oratory and Criticism, on History and General Policy, on Civil Law, and on Anatomy. About this time, too, he made the friendship of Benjamin Franklin—a friendship which constitutes a turning-point in Priestley's career, for Franklin encouraged his leaning towards philosophical pursuits, warmly recommending him to undertake his proposed History of Electricity, and furnishing him with books for the purpose. In connection with this work, he made a number of original observations in electricity, on account of which the book was favourably received; its author was made a Fellow of the Royal Society, and a Doctor of Laws of Edinburgh University. Priestley by this time was married, but seeing no prospect of providing for his family at Warrington, he accepted an invitation to take charge of a congregation in Leeds, and thither he removed in 1767 Having leisure, he redoubled his attention to experimental philosophy, and began that brilliant series of discoveries by which others were to accomplish the overthrow of that system of chemical philosophy of which he considered himself the special champion. "But," writes Priestley, "the only person in Leeds who gave much attention to my experiments was Mr. Hey, a surgeon. . . . When I left Leeds he begged off me the earthen trough in which I had made all my experiments on air while I was there. It was such an one as is there commonly used for washing linen."

In 1772 Lord Shelburne wished for a "literary

companion," and Priestley was induced to accept the office by the offer of a good salary, a house and other appointments, together with an annuity at the end of the engagement. Fortunately for science, his lordship had scarcely any duties for his literary companion to perform, and Priestley was thus able to give most of his time to the continuation of his chemical work. He remained with Lord Shelburne seven years.

He then settled in Birmingham, and accepted the charge of a congregation which he characterises as the most liberal in England. He was now nearly sixty years of age, free from embarrassment of every kind, and happy in the friendship of such men as Boulton and Watt, the engineers; Wedgwood, the potter; Keir, Withering, Darwin, and the Galtons. He had ample leisure for his work, and no lack of encouragement and substantial help when needed. The picture of his life which he draws at this time indicates his serenity of mind and his sense of rest. He is thankful to that good Providence which always took more care of him than he ever took of himself, and he esteems it a singular happiness to have lived in an age and country in which he had been at full liberty both to investigate, and, by preaching and writing, to propagate religious truth. This calm, however, was but the presage of a great storm, and it burst over the old philosopher during the loud strife of party passion which agitated this country at the outbreak of the French Revolution. On the occasion of a public dinner on the anniversary of the taking of the Bastile, at which dinner Priestley was not present, and with which it does not appear that he had anything to do, a mob attacked and wrecked, in the name of "Church and King," the chapels and houses of the Dissenters in the town. The full fury of the rising

seemed to be concentrated upon Priestley, and he and his family barely escaped with their lives, leaving library, papers, and instruments to the tender mercies of the insane crowd, who speedily demolished what had been the labour and fruit of years. Priestley with difficulty got to London, but so uncertain was the temper of the time that his friends forcibly kept him in hiding for some weeks. His appeal for redress met with but a tardy acknowledgment, and the recompense which he eventually received was absurdly disproportionate to his disastrous experience of what Mr. Pitt was pleased to call "the effervescence of the public mind." His sons, disgusted with the justice which he received, left the country, and eventually settled in America. Although he himself was not without a position, for he was invited to minister to a large congregation at Hackney before he had been many months in London, and his friends vied with each other in rendering him help, his situation was still hazardous: his scientific brethren turned their backs upon him, his servants feared to remain with him, and the tradespeople declined to have his custom. At length he determined to follow his sons. Before he left he wrote these remarkable words: "I cannot refrain from repeating again, that I leave my native country with real regret, never expecting to find anywhere else society so suited to my disposition and habits, such friends as I have here (whose attachment has been more than a balance to all the abuse I have met with from others), and especially to replace one particular Christian friend, in whose absence I shall, for some time at least, find all the world a blank. Still less can I expect to resume my favourite pursuits with anything like the advantages I enjoy here. In leaving this country I also abandon a source of maintenance

which I can but ill bear to lose. I can, however, truly say that I leave it without any resentment or ill-will. On the contrary, I sincerely wish my countrymen all happiness; and when the time for reflection (which my absence may accelerate) shall come, they will, I am confident, do me more justice. They will be convinced that every suspicion they have been led to entertain to my disadvantage has been ill-founded, and that I have even some claim to their gratitude and esteem. In this case I shall look with satisfaction to the time when, if my life be prolonged, I may visit my friends in this country; and perhaps I may, notwithstanding my removal for the present, find a grave (as I believe is naturally the wish of every man) in the land that gave me birth." He never returned. His sons had settled at Northumberland, a little town placed in one of the most beautiful spots on the Susquehanna. Here, surrounding himself with books and taking but little interest in the politics of the country, he occupied himself to the last with philosophy and his beloved theology; steadily refusing to become naturalized, although the expediency of such a step was frequently pressed upon him, saying that "as he had been born and lived an Englishman he would die one, let what might be the consequence."

Priestley is mainly remembered by his theological controversies and his contributions to the history of pneumatic chemistry. I have nothing to tell you of his merits as a controversialist, except to say that some of his argumentative pieces are among the most forcible and best written of his literary productions. It is on his chemical work that his reputation will ultimately rest: this will continue to hand down his name when all traces of his other labours are lost. He has frequently

been styled the *Father of Pneumatic Chemistry*; and although we may question the propriety of the appellation when we call to mind the labours of Van Helmont, of Boyle, and of Hales, there is no doubt that Priestley did more to extend our knowledge of gaseous bodies than any preceding or successive investigator.

Priestley was born just as Stahl, the author of what is known in the history of chemistry as the *Phlogistic Theory*, had run out his course. To this theory, handed down as it seemed to his especial keeping, Priestley unswervingly adhered. But, by a strange perversity of fate, the very discoveries which he brought forward as the strongest proofs of the soundness of the Phlogistic doctrine have conduced, perhaps more than any other set of facts, to its destruction. Let me attempt to give you some other notion of this Phlogistic Theory. A piece of wood burns: a piece of stone does not. Why is this? "Because," answers Stahl, "the wood contains a peculiar principle—the principle of inflammability: the stone does not. Coal, charcoal, wax, oil, phosphorus, sulphur—in short, all combustible bodies—contain this principle in common: to this principle (which, indeed, I regard as a material substance) I give the name of *Phlogiston*. I regard all combustible bodies, therefore, as compounds, and one of their constituents is this phlogiston: the differences which we observe in combustible substances depend partly upon the proportion of the phlogiston they contain, and partly upon the nature of the other constituents. When a body burns it parts with its phlogiston; and all the phenomena of combustion —the heat, the light, and the flame—are due to the violent expulsion of that substance. This phlogiston lies at the basis of all chemical change: all chemical reactions are so many manifestations of parts played by

phlogiston." If zinc be strongly heated it takes fire and burns with a beautiful greenish flame, and a white or yellowish-white substance remains behind. "Phlogiston," says Stahl, "is here making its escape. Zinc is composed of phlogiston and the white earthy powder—which I term *calx* of zinc—which now becomes visible." If I melt some lead, and keep it well stirred, it gradually becomes converted into a powder, first of a yellow and ultimately of a beautiful red colour. Phlogiston has thus been gradually expelled, its expulsion having been promoted by stirring the mass, and the calx of lead—the other constituent of the metal—becomes evident. To remake the metal it is merely necessary to impart phlogiston to the calx, and any substance that will give up its phlogiston may be employed for that purpose. If the red lead or the calx of zinc be heated with wood or charcoal, or resin, or phosphorus, or sulphur, the respective metals will be regenerated. Too much of the phlogiston, however, will destroy the metallic nature of the lead or the zinc. If we employ an excess of phosphorus or sulphur (bodies very rich in phlogiston, as their excessive inflammability shows) the metals will combine with the superabundant phlogiston and lose their metallic character.

I told you that in heating the lead the calx had, to begin with, a yellow colour, and that it only became red by the prolonged action of the fire. The change in the colour affords a measure of the rate of the expulsion of the phlogiston. When in the yellow stage the calx has not parted with the whole of the phlogiston: as we continue to heat it more phlogiston is expelled, and the mass becomes red. So, too, if, in performing the reverse operation, we add an insufficient amount of phlogiston, the red calx is not converted into metal—it is only brought back to the yellow stage. In some such

manner as this the Stahlian doctrine attempted to account for the colours of substances.

We all know that if a candle is burnt in a limited amount of air the flame will shortly be extinguished, although no change apparently takes place in the air. This was explained, according to Stahl's doctrine, by supposing that air had an affinity for phlogiston, and that in the act of combustion the phlogiston was transferred from the candle to the air. Gradually, however, the limited amount of air becomes saturated with phlogiston—that is, wholly phlogisticated—and combustion accordingly ceases. In like manner, if a mouse is placed in a confined volume of air, after a time it experiences difficulty in breathing and eventually is suffocated, although the bulk of the air remains the same. The act of breathing, therefore, is nothing else than the transference of phlogiston from the animal to the air, which gradually becomes phlogisticated and is thereby unable to support respiration. To this doctrine of phlogiston, originally broached as a theory of combustion and gradually extended into a theory of chemistry, nearly every European chemist for upwards of half a century after its author's death gave an implicit adherence.

Priestley, whilst at Leeds, lived near a brewery: it was this circumstance that first directed his attention to chemical matters. He had read of *fixed air*, the gas which we now style carbon dioxide or carbonic acid; and being desirous of making himself acquainted with its properties, he took advantage of the fermentative process in which it is abundantly formed to procure some. Priestley at this time had little or no knowledge of chemistry; he was possessed of no apparatus, and had scarcely the means of procuring any. But these

very circumstances were the sources of his success, since he was under the necessity of devising original processes and appliances suited to his narrow means and peculiar views. "If," he says, "I had been previously accustomed to the usual chemical processes, I should not have so easily thought of any other, and without new modes of operation I should hardly have discovered anything materially new." One of the earliest pieces of apparatus which he devised is the well-known *pneumatic trough* —a simple enough piece of chemical furniture certainly, but one that required a considerable amount of experimenting with before it took its present shape. In his experiments with fixed air he observed that this gas conferred "a pleasant acidulous taste" on water, so that he was able in two or three minutes to make a "glass of exceedingly pleasant sparkling water, which could hardly be distinguished from very good Pyrmont, or rather seltzer water." He likewise observed that "the pressure of the atmosphere assists very considerably in keeping fixed air confined in water. . . . I do not doubt, therefore, but that, by the help of a condensing engine, water might be much more highly impregnated with the virtues of the Pyrmont spring; and it would not be difficult to contrive a method of doing it." Priestley here throws out the idea of the manufacture of "soda water"—"a service," says Mr. Huxley, "to naturally, and still more to artificially, thirsty souls, which those whose parched throats and hot heads are cooled by morning draughts of that beverage, cannot too gratefully acknowledge."

Priestley was next attracted by the singular properties of hydrogen, or *inflammable air*, as it was then termed—a gas which had already been made the subject of an elaborate memoir by Mr. Cavendish. Cavendish

was inclined to suppose that inflammable air was phlogiston in the free state—an opinion contrary to the belief of Stahl and his immediate followers, who imagined that phlogiston was a solid earthy volatile substance. In order to get some clue as to the nature of this protean body, Priestley placed a quantity of minium or the *calx of lead*—that is, lead from which the phlogiston has been expelled—within a tall cylinder, filled with inflammable air, and standing over water. He then proceeded to heat the calx by means of a burning lens —a method which he constantly employed, and which materially contributed to many of his discoveries. Let us give the result in his own words: "As soon as the minium was dry, by means of the heat thrown upon it, I observed that it became black, and then ran in the form of perfect lead; at the same time that the air diminished at a great rate, the water ascending within the receiver. I viewed this process with the most eager and pleasing expectation of the result, having at that time no fixed opinion on the subject; and therefore I could not tell except by actual trial whether the air was decomposing in the process, so that some other kind of air would be left, or whether it would be absorbed *in toto*. The former I thought the more probable, as if there was any such thing as phlogiston, inflammable air, I imagined, consisted of it and something else. However, I was then satisfied that it would be in my power to determine, in a very satisfactory manner, whether the phlogiston in inflammable air had any *base* or not; and if it had, what that base was. For, seeing the metal to be actually revived, and that in a considerable quantity, at the same time that the air was diminished, I could not doubt but that the calx was actually imbibing something from the air; and

from its effects in making the calx into metal, it could be no other than that to which chemists had unanimously given the name of *phlogiston*."

This experiment he repeated with every precaution, and in every conceivable manner—varying the nature of the calx, sometimes taking the calx of tin, of bismuth, of mercury, of silver, of iron, and of copper—and sometimes making the experiment over quicksilver instead of water. He found that the inflammable air was totally absorbed; and, accordingly, he concludes—"that phlogiston is the same thing as inflammable air, and is contained in a combined state in metals, just as fixed air is contained in chalk and other calcareous substances: both being equally capable of being expelled again in the form of air."

Priestley then proceeded to determine the amount of the phlogiston which must be contained in the various metals, by ascertaining the quantity of inflammable air taken up by their calces. He found that 1 oz. of lead was revived by the absorption of 108 oz. measures of inflammable air, and 1 oz. of tin by the absorption of 377 oz. measures. Let me direct your attention for a moment to these numbers, since they afford us a ready means of determining the degree of accuracy with which Priestley made his observations. The 108 oz. measures of hydrogen required to revive the 1 oz. of lead are equivalent to 204·1 cubic inches, and weigh, at the ordinary temperature, about 4·4 grains. Now, the most refined processes of modern chemical analysis have shown that the weight of hydrogen required to regenerate 1 oz. of lead from the yellow calx is 4·6 grains—no great disparity, after all, from Priestley's result. The 377 oz. measures of hydrogen required to revive 1 oz. of tin would weigh about 15·4 grains; modern chemistry

says that the exact quantity needed is 16·3 grains. Priestley was here on the verge of a great discovery—a discovery which, in the first place, would have given a crushing blow to Stahl's doctrine—and which, in the second, might have ended in the determination of a fact of no less magnitude than the true composition of water. But his phlogistic ideas rendered him blind to the full significance of his results. He was prepossessed with the notion that by phlogisticating the calx it gained in weight, and that the weight of the metal formed must be equal to the weight of the calx *plus* that of the phlogiston absorbed. He tells us that he frequently attempted to ascertain the weight of the inflammable air in the calx, "so as to prove that it had acquired an addition of weight by being metallized," but the result never came out in accordance with the theory. This, he satisfies himself, must be due to part of the calx subliming, and part being dissolved by the mercury; and he concludes, "that were it possible to procure a perfect calx, no part of which should be sublimed and dispersed by the heat necessary to be made use of in the process, I should not doubt but that the quantity of inflammable air imbibed by it would sufficiently add to its weight." Every sound phlogistian for at least a quarter of a century after Stahl's death believed that when a metal was calcined the calx *must* weigh less than the metal: for had not phlogiston been expelled? There were indeed certain vague rumours that various people had found it otherwise: Boyle had made some experiments with tin; a French surgeon named Rey had experimented upon lead; and an obscure alchemist called Sulzbach had recorded some observations upon mercury; but then these people had not had the good fortune to work in the light of the phlogistic doctrine,

or they were sceptics who were justly punished for their unbelief by their false results. But about Priestley's time it gradually dawned upon the phlogistians that the sceptics and ignorant people might be right after all, for some of their own trusted number had condescended to repeat the experiments which so obstinately refused to chime in with the established order of things, and found, doubtless to their dismay, that it could no longer be gainsaid that a metal by calcination *gained* in weight. But the phlogistians were not going to see their beautiful superstructure—a theory in which all the parts seemed to fit so nicely—brought ignominiously down by the trivial weight of such a fact as this. We concede, said they, that we have been in error respecting the precise nature of phlogiston : it cannot be the gross earthy substance that Stahl had taught us to believe in. It is plainly something far more etherealised—a sort of invisible, imponderable ether—the very *principle of levity*, in fact, a principle so very light that so far from adding to the weight of bodies with which it combines, it actually makes them lighter than they were before! It seems scarcely credible, but this was precisely the position taken up by a large section of the phlogistians ; not by all of them, however, for some were sagacious enough to see that a theory which needed a hypothesis of this character to bolster it up must be rapidly on the wane. "Of late," writes Priestley, "it has been the opinion of many celebrated chemists, Mr. Lavoisier among others, that the whole doctrine of phlogiston is founded on mistake. The arguments in favour of this opinion, especially those which are drawn from the experiments Mr. Lavoisier made on mercury,[1] are so specious that I own I was myself much inclined to adopt

[1] A repetition of the experiments of Sulzbach.

it." And Priestley assuredly would have adopted it if he could only have looked at the results of his experiments otherwise than through the fogs of his prejudices. He would have grasped the fact that with the disappearance of ponderable inflammable air (for light as it is it could not have been the principle of levity), the calx *lost* weight, and by much more than the weight of the inflammable air. This fact once properly laid hold of might have explained the origin of that water which he distinctly noted as being produced in his trials over mercury. In one of his experiments he heated a quantity of the calx of mercury in inflammable air, and although, as he tells us, "the gas was previously well dried with fixed ammoniac," water was found in "sufficient quantity." "This experiment," he goes on to say, "may be thought to be favourable to the hypothesis of water being composed of fixed and inflammable air: as all water was carefully excluded, and yet a sufficient quantity was found in the process." But to the notion of the compound nature of water he attaches no weight. The water he supposes came either from the calx or, which he thinks more probable, from the inflammable air—that it was in fact essential to the constitution of the gas; an opinion which became a conviction when he observed how frequently water was formed in processes in which the inflammable air played a part.

When steam is driven through a red-hot iron tube inflammable air, the phlogiston of Priestley and Cavendish, is produced in abundance—a fact first observed by Lavoisier; but then, as Priestley says, "Mr. Lavoisier is well known to maintain that there is no such thing as what has been called *phlogiston*; affirming inflammable air to be nothing else but one of the elements or constituent parts of water. As to myself, I was a long

time of opinion that his conclusion was just, and that the inflammable air was really furnished by the water being decomposed in the process. But though I continued to be of this opinion for some time, the frequent repetition of the experiments, with the light which Mr. Watt's observations threw upon them, satisfied me, at length, that the inflammable air came from the iron." The arrangement which Priestley made use of in these experiments is identical with that which we use on our lecture tables to-day for the same purpose. Steam is driven through an iron tube heated to redness, and the inflammable air is collected in one of Priestley's pneumatic troughs. "Of the many experiments which I made with iron," says Priestley, "I shall content myself with reciting the following results. With the addition of 267 grains to a quantity of iron, and the loss of 336 grains of water, I procured 840 ounce measures of inflammable air; and with the addition of 140 grains to another quantity of iron, and the consumption of 254 grains of water, I got 420 ounce measures of air." These numbers again serve to test the accuracy of Priestley's work. In the first experiment the iron gained 267 grains, and the yield of inflammable air was 840 ounce measures. 840 ounce measures of hydrogen, at the ordinary temperature, weigh 34·3 grains; that is, the gain of the iron was $7\frac{3}{4}$ times the weight of the inflammable air. Assuming, then, with Lavoisier, that water is a compound, and that one constituent is fixed by the iron and the other makes its escape as inflammable air, it would follow from Priestley's experiment that water is composed of $7\frac{3}{4}$ parts by weight of the substance fixed by iron, united to 1 part by weight of inflammable air. Modern science has completely established the correctness of Lavoisier's opinion, and disproved

that of Priestley, but it has added little, even with all its elaborate processes of quantitative analysis, to the results of Priestley's trials. Water is composed of oxygen—the substance fixed by the iron—and inflammable air, or hydrogen; and the proportion by weight of the former gas to the latter is almost exactly as 7·9 to 1.

Acting upon some remarks by Mr. Cavendish, Priestley was led to study the action of *aqua fortis*, or "nitrous acid," as it was then called, upon the metals. Trying first upon brass, and then upon copper, he obtained a gas to which he gave the name of *nitrous air*, but which is now called *nitric oxide*. "One of the most conspicuous properties of this kind of air is the great diminution of any quantity of common air with which it is mixed, attended with a turbid red, or deep orange colour, and a considerable heat. . . . The diminution of a mixture of this and common air is not an equal diminution of both the kinds . . . but of one-fourth of the common air, and as much of the nitrous air as is necessary to produce that effect. . . . I hardly know any experiment that is more adapted to amaze and surprise than this is, which exhibits a quantity of air, which, as it were, devours a quantity of another kind of air half as large as itself, and yet is so far from gaining any addition to its bulk, that it is considerably diminished by it. It is exceedingly remarkable that this effervescence and diminution, occasioned by the mixture of nitrous air, is peculiar to common air, *or air fit for respiration*, and, as far as I can judge from a great number of observations, is at least very nearly, if not exactly, in proportion to its fitness for this purpose; so that by this means the goodness of air may be distinguished much more accurately than it can be done by

putting mice, or any other animal, to breathe in it." Upon this principle Priestley devised a method of measuring the quality of air. A small phial, termed the *air measure*, about an ounce in capacity, was filled with the air to be examined, which was then transferred to a jar about $1\frac{1}{2}$ inches in diameter, previously filled with water. The air measure was then filled with the nitrous air and emptied into the jar containing the air to be analysed. The mixture was allowed to stand for about two minutes, and was then transferred to a glass tube about two feet long and one-third of an inch wide, graduated in terms of the air measure, and divided into tenths and hundredth parts. The volume of the residual gas was then read off, care being taken to immerse the tube to such a depth in the trough that the water in the inside and on the outside was on the same level. The result was expressed in measures and parts of a measure: thus, if on mixing equal volumes of common air and nitrous air the residual volume was one measure and two-tenths of a measure, the standard of the air was said to be 1·2.

With this instrument Priestley attempted to measure the difference between good air and that which was reputed to be unwholesome; but, although he compared the worst air he could get from manufactories, from coalpits, and from the holds of ships, with the best country air, he was unable to perceive any difference; and he was satisfied, therefore, "that air may be very offensive to the nostrils, probably hurtful to the lungs (and, perhaps, also in consequence of the presence of phlogistic matter in it), without the phlogiston being so far *incorporated with it* as to be discoverable by the mixture of nitrous air. . . . I have frequently taken the open air in the most exposed places in the country, at

different times of the year and in different states of the *weather*, etc., but never found the difference so great as the inaccuracy arising from the method of making the trial might easily amount to or excel." Other experimenters, less conscientious than Priestley, found the differences they sought for; but the researches of Bunsen, of Regnault, and of Dr. Angus Smith, made with all the precision of modern gasometric analysis, have shown that the atmosphere is wonderfully constant in composition, and that, although there are variations, they are infinitely beyond the cognisance of the nitrous air test.

A second observation by Mr. Cavendish led Priestley to another discovery. Cavendish, in the course of the work on inflammable air to which I have alluded, attempted to prepare that gas by acting on copper with spirit of salt, or "marine acid," as it was then commonly called. Instead of the wished-for result, he procured "a much more remarkable kind of air, viz. one that lost its elasticity by coming in contact with water." By substituting quicksilver for water in his trough, Priestley obtained this air in quantity, and examined its properties. He quickly found that the copper played no part in the process of making the gas, for on heating the acid alone he procured it just as readily. "So that," he says, "this remarkable kind of air is, in fact, nothing more than the vapour, or fumes of spirit of salt, which appear to be of such a nature that they are not liable to be condensed by cold, like the vapour of water and other fluids; and therefore may be very properly called an *acid air*, or more restrictively, the *marine acid air*." Spirit of salt, or, as chemists also term it, hydrochloric acid, is therefore nothing else than a solution of Priestley's marine acid air in water.

This discovery induced Priestley to try the same experiment with other acids, and, among them, with oil of vitriol. But he says, "I got no air from the oil of vitriol by any application of heat. But in attempting to procure it, I got it by means of *mercury* in a manner that I little expected, and I paid pretty dearly for the discovery it occasioned. Despairing to get any air from the longer application of my candles, I withdrew them; but before I could disengage the phial from the vessel of quicksilver, a little of it passed through the tube into the hot acid, when instantly it was all filled with dense white fumes, a prodigious quantity of air was generated, the tube through which it was transmitted was broken into many pieces (I suppose by the heat that was suddenly produced), and part of the hot acid being spilled upon my hand burned it terribly, so that the effect of it is visible to this day. The inside of the phial was coated with a white saline substance, and the smell that issued from it was extremely suffocating. . . . Not discouraged by the disagreeable accident above mentioned, the next day I put a little quicksilver into the phial along with the oil of vitriol, when, before it was boiling hot, air issued plentifully from it." The new gas with which Priestley was rewarded for his pain and perseverance he termed *vitriolic acid air*: it is now known as sulphur dioxide, and is precisely the same substance which is produced on burning brimstone in the air. You have doubtless all noticed its formation on striking an old-fashioned lucifer match.

I daresay many of you have seen the beautiful etchings made upon glass by means of *hydrofluoric acid* —an acid first obtained by a contemporary of Priestley, named Scheele—a poor Swedish apothecary, and one of the greatest chemists of the 18th century. Glass, as you

are doubtless aware, is a mixture of sand or silica, lime, alkali, and occasionally red lead. The hydrofluoric acid acts upon the glass by seizing upon the silica and forming with it a gaseous substance termed by chemists *fluoride of silicon*. This fluoride of silicon was obtained by Priestley by heating a mixture of fluor spar, or Derbyshire spar, with oil of vitriol in a glass vessel. When this gas (which he termed *fluor acid air*) is led into water it is instantly decomposed, and silica is reproduced. The formation of this silica constitutes a very striking experiment; so much so, that, says Priestley, "I have met with few persons who are soon weary of looking at it, and some could sit by it almost a whole hour and be agreeably amused all the time."

I doubt not that you are all familiar with that pungent, tear-exciting liquid termed by the apothecaries "spirits of hartshorn," or ammonia. This substance has been known for a very long time: its name, "ammonia," is derived from the circumstance that it was prepared, ages ago, by the Arabs in the desert near the temple of Jupiter Ammon. Now, although this liquid has been known for some thousands of years, it required Priestley to tell us that its peculiar properties were due to a gas held in solution. Priestley treated the spirit of hartshorn as he had treated the spirit of salt, and he presently found that a great quantity of a transparent and, apparently, permanent air was discharged from it. He ascertained all the more striking attributes of this "alkaline air," as he termed it; among others, its solubility in water and its inflammability. He next proceeded to determine its composition by passing electric sparks through it, and he found that, after passing the sparks until no further increase of bulk could be observed, the gas was ultimately trebled in

volume, and that no part of it was soluble in water. The gas, in fact, had been decomposed into its constituents—into hydrogen (the presence of which Priestley recognised), and into nitrogen, which he calls phlogisticated air, and which, he says, is contained to the extent of one-fourth of the bulk of the mixture. He then tried the action of the alkaline air upon the airs which he had previously discovered, and notably upon the "marine acid air," as he had "a notion that these two airs, being of opposite natures, might compose a *neutral air*, and perhaps the very same thing with common air. But the moment that these two kinds of air came into contact a beautiful white cloud was formed, and there appeared to be formed a solid *white salt*, which was found to be the common *sal ammoniac*, or the marine acid united to the volatile alkali."

If by some evil chance the cold and damp of this coming winter should drive some of you to the dentist, and if after seating you in that awful chair and harrowing your distracted nerves with the sight of his murderous tools, he humanely offers to send you to sleep with his nitrous oxide, by all means let him, and, when you wake with the sweet consciousness that "it is all over," give a passing benediction to the memory of Priestley, for he first told us of the existence of that gas.

If, too, as you draw up to the fire "betwixt the gloaming and the mirk" of these dull, cold November days, and note the little blue flame playing round the red-hot coals, think kindly of Priestley, for he first told us of the nature of that flame when in the exile to which our forefathers drove him.

The crowning work of Priestley's life was, however, the discovery of that gas which he termed *dephlogisticated air*, but to which Lavoisier, who swept away all

the jargon of the Phlogistic doctrine, gave the name of Oxygen. The manner of this discovery is characteristic of much of Priestley's work. "It furnishes," he says, "a striking illustration of the truth of a remark which I have more than once made in my philosophical writings, and which can hardly be too often repeated, as it tends greatly to encourage philosophical investigations; viz. that more is owing to what we call *chance*, that is, philosophically speaking, to the observation of *events arising from unknown causes*, than to any proper *design* or preconceived *theory* in this business." The accident of possessing a burning glass "of considerable force" led Priestley to try the effect of the heat of the sun upon various substances contained in tubes filled with mercury, and standing over the mercurial trough. "With this apparatus, after a variety of other experiments, an account of which will be found in its proper place, on the 1st of August 1774 I endeavoured to extract air from *mercurius calcinatus per se*—that is, calx of mercury, and I presently found that, by means of this lens, air was expelled from it very readily. Having got about three or four times as much as the bulk of my materials, I admitted water to it, and found that it was not imbibed by it. But what surprised me more than I can well express was, that a candle burned in this air with a remarkably vigorous flame, very much like that enlarged flame with which a candle burns in nitrous gas exposed to iron or liver of sulphur [that is, his nitrous oxide gas]; but as I had got nothing like this remarkable appearance from any kind of air besides this particular modification of nitrous air, and I knew no nitrous air was used in the preparation of *mercurius calcinatus*, I was utterly at a loss how to account for it." His astonishment was still further increased when

he found that, tested with his nitrous air, the new gas was actually better than common air, and that mice would live longer in it than in an equal bulk of that air. He had the curiosity to breathe it himself. "The feeling of it to my lungs was not sensibly different from that of common air; but I fancied that my breast felt peculiarly light and easy for some time afterwards. Who can tell but that in time this pure air may become a fashionable article in luxury? Hitherto only two mice and myself have had the privilege of breathing it. . . . But, perhaps, we may also infer from these experiments, that though pure dephlogisticated air might be very useful as a *medicine*, it might not be so proper for us in the usual healthy state of the body; for, as a candle burns out much faster in dephlogisticated than in common air, so we might, as may be said, *live out too fast*, and the animal powers be too soon exhausted in this pure kind of air. A moralist, at least, may say, that the air which nature has provided for us is as good as we deserve."

Priestley at length got to the conclusion that common air was no longer a "*simple elementary substance*, indestructible and unalterable," but that it was composed of 1 volume of his new air and 4 volumes of phlogisticated air. This new air, he concluded, was devoid of phlogiston—hence the term "dephlogisticated air," but that in the processes of respiration and combustion phlogiston was imparted to it. Priestley found that he could obtain this air from the calx of lead as well as from the calx of mercury, and this fact, he says, "confirmed me more in my suspicion that the *mercurius calcinatus* must have got the property of yielding this kind of air from the atmosphere, the process by which that preparation, and this of red lead, is made being similar. As I never make the least secret of anything

that I observe, I mentioned this experiment also, as well as those with the *mercurius calcinatus*, to all my philosophical acquaintances at Paris and elsewhere, having no idea at that time to what these remarkable facts would lead." The knowledge which Priestley, as he tells us, imparted to the French chemists was used by them with crushing effect against his favourite theory. The discovery of oxygen was the deathblow to phlogiston. Here was the thing which had been groped for for years, and which many men had even stumbled over in the searching, but had never grasped. Priestley indeed grasped it, but he failed to see the magnitude and true importance of what he had found. It was far otherwise with Lavoisier. He at once recognised in Priestley's new air the one fact needed to complete the overthrow of Stahl's doctrine; and now every stronghold of phlogistonism was in turn made to yield. Priestley, however, never surrendered, even when nearly every phlogistian but he had given up the fight or gone over to the enemy. When age compelled him to leave his laboratory he continued to serve the old cause in his study, and almost his last publication was his *Doctrine of Phlogiston Established.* His own life, indeed, affords an exemplification of the truth of his own words, that "we may take a maxim so strongly for granted, that the plainest evidence of sense will not entirely change, and often hardly modify, our persuasions; and the more ingenious a man is, the more effectually he is entangled in his errors, his ingenuity only helping him to deceive himself by evading the force of truth."

III

CARL WILHELM SCHEELE

AN ADDRESS TO THE OWENS COLLEGE CHEMICAL SOCIETY, AT THE OPENING MEETING, 24TH OCTOBER 1893; SUBSEQUENTLY PUBLISHED IN THE *FORTNIGHTLY REVIEW*.

IN the personal history of learning there are few more striking or, in a sense, more romantic figures than the chemist Scheele. "La vie de M. Scheele," wrote Vicq d'Azyr, "offre l'exemple d'un savant modeste qui, dédaignant tout éclat, eut le courage de vivre obscur; dont le zèle n'eut pas besoin d'être excité par la louange, et qui, connu des gens de l'art, mais presque ignoré de son siècle, avoit rendu son nom immortel lorsqu'il n'avoit pas encore de célébrité."[1] An obscure apothecary, living a solitary sedentary life in a small town on the shore of a Scandinavian lake, hampered by poverty and harassed by debt, hypochondriacal, and, at times, the victim of the most depressing melancholy—he yet succeeded by the sheer force of his genius as an experimentalist, and under the influence of a passion which defied difficulty and triumphed over despair, in changing the entire aspect of a science. No man ever served chemistry more loyally or with a purer, nobler, more disinterested devotion than Scheele. "Diese edel Wissenschaft," he wrote to his friend Gahn, "ist mien Auge."

[1] *Éloges historiques*, vol. ii. p. 19.

The pursuit of truth for its own sake—with no thought of worldly gain or reward—was to him the supreme object of his existence and the highest form of his religion. The cause of science was, indeed, as sacred to him as if it were that of a martyr, and he gave up his life to her service with a martyr's spirit of patience, self-sacrifice, and humility.

But although Scheele's name is associated with some of the most remarkable discoveries of the eighteenth century, and of which the value was quickly recognised by his contemporaries, comparatively little is known of his personal characteristics, of his habits of work, or of the nature of his surroundings. Practically the only mental picture of him that we have hitherto been able to form is to be derived from the memorial notice of him by Sjösten, the Secretary of the Stockholm Academy of Sciences, which appears in the *Proceedings* of the Academy for 1799, that is thirteen years after Scheele's death. Sjösten was not a chemist, and was otherwise unfitted to judge of the merit and true proportion of Scheele's work. He appears to have obtained his information from materials collected by his predecessor in office, Johan Carl Wilcke, whose name is honourably known in the history of science from his connection with the discovery of latent heat. On the death of Scheele, Wilcke placed his papers and laboratory notes in the charge of the Academy, which subsequently came into possession of Scheele's correspondence with Retzius, Gahn, and Hjelm. From this rich material, together with a collection of letters to Bergmann, preserved in the University of Upsala, Wilcke conceived the idea of preparing an account of Scheele's life and labours which should set forth the origin and chronological history of his investigations, and so exhibit his true relations as

a discoverer to his great contemporaries, Cavendish, Priestley, and Lavoisier. Unfortunately the realisation of this project was frustrated by Wilcke's death. Thanks, however, to the piety and patriotism of Baron Nordenskiöld this valuable collection of letters and laboratory memoranda has now been given to the world, and the historian of chemistry is at length in a position to determine much in Scheele's life that has hitherto been doubtful and obscure.[1]

M. Nordenskiöld has been materially aided in his work by the *Lars Hiertas minne* Trust, and, above all, by the zeal of Mme. Elin Bergsten, who undertook not only to transcribe the letters, which are difficult to read on account of their archaic style and antiquated language and the constant employment in them of an obsolete nomenclature, but also to decipher the laboratory notes, which are for the most part rough jottings of experimental results put together by means of contractions and a system of symbols wellnigh as illegible as that of the alchemists.

The handsome well-printed volume which embodies the results of so much patient and conscientious labour has appeared at a timely moment; indeed, no more fitting memorial of the one hundred and fiftieth anniversary of the birth of the great Swedish chemist could be conceived than the publication of a work which fixes for all time, without question or cavil, his true relation to his epoch, and his place in the history of scientific discovery. Scheele, who took little thought for his own fame, owes much to women; for, it is worth noting, Mme. Bergsten is not the first of her sex who has striven to perpetuate his genius. It was through Mme. Picardet,

[1] *Carl Wilhelm Scheele: Nachgelassene Briefe und Aufzeichnungen.* Herausgegeben von A. E. Nordenskiöld. Stockholm: Verlag von P. A. Norstedt & Söner.

the wife of a magistrate at Dijon, that France first gained a knowledge of his memoirs. Instigated by De Morveau, she learned German and Swedish solely for the purpose of translating Scheele's papers.

Carl Wilhelm Scheele was born on 9th December 1742 at Stralsund, at that time the capital of Swedish Pomerania. He was the seventh child in a family of eleven. His father, Joachim Christian Scheele, was a merchant of some note in Stralsund. He came of a good stock, branches of which had occupied important positions in North Germany as far back as the fifteenth and sixteenth centuries. One member became Bishop of Lübeck, and another distinguished himself as an admiral in the Swedish service in the time of Charles XI. A female connection of the family, Anna Scheele, was the mother of Wilcke, the Secretary of the Swedish Academy of Sciences, whose name has already been mentioned as having projected a biography of his illustrious relative. The Stralsund merchant was apparently not in a position to afford his sons the advantages of a university training. Carl Wilhelm was placed at a private school in his native town, and after having acquired a fair knowledge of Latin he passed on to the gymnasium. He seems to have been a thoughtful, studious boy, remarkable among his fellows for diligence and for the ease and rapidity with which he accomplished his school tasks. The bent of his mind towards science would appear to have manifested itself even at this time; at all events, he then acquired that facility in writing chemical symbols which characterised his letters and memoranda, and the apothecary Cornelius, who gave him instruction in reading pharmaceutical receipts and prescriptions, has testified to his aptitude for chemical study and speculation. It is not improbable,

however, that the course of his inclination may have been, to some extent, directed from home. His eldest brother, Johann Martin, had been apprenticed to an apothecary in Gothenburg named Bauch, but had died whilst Carl Wilhelm was at school. Three years afterwards, that is when fourteen years of age, he too was apprenticed to Bauch. The Gothenburg apothecary seems to have been an honest, even-handed man, who, to judge from the inventory of his possessions in the archives of the Rathhaus of the town, followed his calling in a worthy, liberal-minded fashion. In Bauch's laboratory Scheele made the practical acquaintance of nearly all the pharmaceutical and chemical products of his time. He had also access to such standard works as Neumann's *Praelectiones Chemicae*, Lémery's *Cours de Chimie*, Boerhaave's *Elementa Chemicae*, Kunckel's *Laboratorium Chymicum*, and Rothe's *Anleitung zur Chymie*. Nor was he slow to avail himself of his opportunities. Bauch, in letters to the Stralsund home, fears for the health of his young charge, who devotes hours which should be given to sleep either to the study of books which are beyond his years, or to the making of experiments that would tax the skill of his older fellow-apprentices. Kunckel's *Laboratorium* and Neumann's *Chymie* seem, indeed, to have been his chief instructors in practical chemistry, and it was by diligently repeating the experiments contained in these books that he laid the foundations of the manipulative skill and analytical dexterity on which his success as an investigator ultimately rested.

In 1765 Bauch, then an old man, sold his business, and Scheele, now twenty-three years of age, took service with Kjellström, an apothecary in Malmö, with whom he remained about a couple of years.

Kjellström has recorded his opinion of his young assistant, but it is from his fellow-worker and friend Retzius that we derive the most vivid conception of Scheele at this period of his career. Anders Johan Retzius was of the same age as Scheele, and, like him, began life as a pharmacist. Eventually he attached himself to the University of Lund, as director of its Museum and Botanical Garden, and died at Stockholm in 1821, the last survivor of the Phlogistic School of Chemists. In a communication found amongst Wilcke's papers Retzius thus records his impressions of Scheele :

His genius was wholly concerned with physical science. He had absolutely no interest in any other. . . . Although possessing an excellent memory, it seemed only fitted to retain matters relating to chemistry.

" One science only will one genius fit," says Pope.

During his stay at Malmö he bought as many books as his small pay enabled him to procure. These he would read once or twice through, when he would remember all that he desired to recall, and never again consulted them. Without systematic training and with no inclination to generalise, he occupied himself mainly with experiments. During the time of his apprenticeship at Gothenburg he worked without plan and for no other purpose than to note phenomena ; these he could remember perfectly. Eleven years' continuous exercise in the art of experimenting had enabled him to collect such a store of facts that few could compare with him in this respect. In addition he gained a readiness in devising and executing experiments such as is rarely seen. He made all kinds of experiments without reference to any system or prearranged plan. He was thus enabled to learn what no doctrinaire could possibly acquire, since working by no formulated principles he observed much and discovered much that the doctrinaire would consider impossible, inasmuch as it was opposed to his theories. I once persuaded him during his stay at Malmö to keep a journal of his experiments, and, on seeing it, I was amazed, not only at the great number he made, but also at his extraordinary aptitude for the art.

In 1768 Scheele removed to Stockholm, where he superintended the shop of an apothecary named Scharenberg. Here his opportunities for experimenting were considerably restricted. However, a window with a sunny aspect close to his place of work enabled him to make the novel and important observation that different parts of the solar spectrum influence the decomposition of silver chloride in very different degrees. It was about this time that his name first appears in chemical literature as a discoverer. With his friend Retzius he undertook the examination of cream of tartar, and succeeded in isolating, for the first time, its characteristic acid, the properties of which he carefully studied, and from which he was enabled to conclude that it differed from all acids up to that time known.

This, however, was not the first attempt made by Scheele to contribute to the literature of science. Retzius tells us that he had forwarded to the Academy an account of an inquiry into the nature of the so-called *Globuli martiales*, a pharmaceutical preparation made by boiling finely-divided iron with a solution of cream of tartar. The paper was, for the most part, a description of experiments; it was unmethodically put together, and was without definite theoretical result. It was referred by the Academy to Bergmann, and as his opinion was adverse, it was never published, and was ultimately lost. From Scheele's correspondence with Gahn, and from the laboratory memoranda which have now been published, we are able to glean an idea of the contents of this memoir. Some of the observations were unquestionably new and not without importance. Thus Scheele found that hydrogen was evolved by the contact of organic acids with iron, and he describes an apparatus by which this gas may be obtained by the action of

water alone on iron filings. The theoretical value of these facts will be obvious from the circumstance that Cavendish, at that time the recognised authority on hydrogen, or inflammable air, as it was then termed, had stated in his classical papers on "Factitious Air," published in the *Philosophical Transactions* for 1766: "I know of only three metallic substances, namely, zinc, iron, and tin, that generate inflammable air by solution in acids, and those only by solution in the diluted vitriolic acid, or spirit of salt."

Nor was Scheele more fortunate with his second contribution — "Chemical Experiments with Sal-acetosellae" [acid potassium oxalate], which he sent to the Academy in 1768. The paper was read, but was not published—again through the intervention of Bergmann. It is doubtful if Bergmann at this time had any personal knowledge of Scheele; at all events, it is impossible to suppose that he was in any way influenced by animosity. The "hochedelgeborner Herr Professor" to whom Scheele a year or two afterwards subscribed himself as his "dienstschuldigster Knecht," and with whom he was to live in the closest bonds of sympathy and mutual esteem, although one of the most cultivated men of his age, and distinguished by the breadth of his knowledge, which ranged over zoological, physical, and cosmographical science, had at this period little acquaintance with experimental chemistry. It is hardly to be wondered at, therefore, that the crude essays of the unknown apothecary's assistant, who, like Addison's clubfellow, was somewhat awkward at putting his talents within the observation of such as should take notice of them, should have failed to commend themselves to the critical judgment and refined taste of the *homo multarum literarum*,

noted for the grace and polish of his style. There is reason to believe that these disappointments reacted upon the sensitive nature of Scheele, and that the rejection of his papers by the Academy, together with the uncongenial nature of his position in Stockholm, induced him to leave the capital in order to accept employment as a laborant in the pharmacy of Lokk at Upsala. Whatever may have been the real grounds for the change, there is no question that it was attended with the most beneficial results on Scheele's fortunes. To begin with, he was brought into personal contact with Bergmann. This *rapprochement* was due to Gahn, who had made Scheele's acquaintance in Stockholm, and who had been greatly impressed with the power and capacity of the young apothecary. It is said that Bergmann, unable to explain the change that nitre experiences when it is strongly heated, whereby it is converted into the deliquescent potassium nitrite, and evolves a ruddy gas when treated with oil of vitriol, was led by Gahn to consult Scheele, to whom the phenomena and their cause were well known. According to Retzius, the properties of the so-called *Salpeterluft*, as the ruddy gas came to be termed, were ascertained by Scheele when at Malmö, and were known to him long before anything had been written on the subject. This meeting laid the foundation of a warm and active friendship which ended only with Bergmann's death—a friendship, too, which was of the greatest service to science. "It would be difficult to say," wrote Retzius, "which of the two, Scheele or Bergmann, was the teacher or the taught. Bergmann, without a doubt, received the greater part of his practical instruction from Scheele, whilst Scheele owed to Bergmann the wider knowledge of his later years." It was at Bergmann's instigation

that Scheele undertook the epoch-making investigation of *magnesia nigra*, the *Braunstein* or pyrolusite of the German mineralogist, the " wad " of the English miner, whereby he not only showed that this substance contained a metal hitherto unknown, but also incidentally discovered oxygen, chlorine, and baryta. It may seem remarkable that Scheele, with his tastes and aptitudes, should not have followed the example of his friend Retzius, and have abandoned pharmacy for an academic career. M. Nordenskiöld finds an explanation in the assumption that the *Zunftgeist* of the time would not permit of the introduction of the *studiosus pharmaciae* within the academic circle. It is doubtful, however, whether Scheele was at all fitted, either by temperament or training, for an academic career, and as schools of chemistry were at that time constituted it is certain that he would have gained little by the change. Chemical laboratories were seldom to be found at the universities, even at the largest, and the chemical prelections of the period were, for the most part, dull and formal disquisitions unenlivened by a single experimental illustration. On the other hand, the pharmacist at that time had a right to the appellation which, in this country at least, he now too frequently usurps. He was a practical chemist in the real sense of the term, and his laboratory was of more importance to him than his shop.

Whilst with Lokk, Scheele seems to have had abundant opportunity for the prosecution of his inquiries. It was at Upsala that he collected the greater part of the experimental material for his great work on *Air and Fire*. The correspondence and laboratory memoranda which M. Nordenskiöld has given to the world, show that *prior to* 1773, *that is at least a year before the date of*

Priestley's discovery, Scheele had prepared oxygen from the carbonates of silver and mercury, from mercuric oxide, nitre and magnesium nitrate, and by the distillation of a mixture of manganese oxide and arsenic acid. It was at Upsala, too, that he began and finished his work on manganese, chlorine, and baryta; he also demonstrated the acidic character of silica and the chemical nature of magnesia, microcosmic salt, and oxalic acid.

On 4th February 1775, when thirty-two years of age, Scheele was made a member of the Swedish Academy of Sciences, a distinction never accorded, either before or since, to a student of pharmacy. In the following year he was appointed, by the Medical College, *provisor* of the pharmacy at Köping, a small town on the north shore of Lake Mälar, as successor to Hinrich Pohl, whose privilege, in conformity with Swedish law, had passed, on his death, to his young widow, Sara Margaretha Sonneman. Scheele now seemed to himself to have reached the goal of his aspirations; he had at length, he thought, obtained an independent position with the prospect of a fairly lucrative business, and he would now be able to follow his cherished projects under conditions of comparative ease and comfort. "Oh, how happy I am," he wrote to Gahn, "with never a care about eating or drinking or dwelling!" The quiet peaceful life he saw before him was to be consecrated to science. "There is no delight," he wrote, "like that which springs from a discovery; it is a joy that gladdens the heart."

But the haven of rest was not yet won. The young academician, rich in honour, was poor in means, and unlooked-for difficulties arose respecting the transfer of the lease. The widow and her father were exacting,

and other *provisors* came forward who understood the art of money-getting better than Scheele. Scheele's letters seldom contain allusions to his private affairs, but the half-dozen lines in which he makes mention to Gahn and to Bergmann of his disappointment show how deeply he felt it. Offers of assistance came from all sides. Gahn invited him to Fahlun; Bergmann wished him to return to Upsala: "Es fällt uns beiden schwer uns von einander zu trennen," he had written at the prospect of the change to Köping. The suggestion was publicly made that he should be "chemicus regius" in the capital. He had even invitations from abroad. D'Alembert, in a letter to Frederick II., suggested that he should be called to Berlin. "J'ai appris," he wrote, "il y a peu de temps qu'il y avait à Stockholm un très habile chimiste, nommé M. Scheele, Membre de l'Académie des Sciences de cette ville, et qui, sans m'être d'ailleurs connu, me paroit fort estimé par les plus habiles chimistes de la France." Among Wilcke's papers was found a letter from the brother, Fr. Christian Scheele, from which it appears that Scheele actually did receive an invitation to Berlin with a salary of 1200 reichthalers. Crell, the editor of the well-known *Neue Entdeckungen* and *Annalen* in which many of Scheele's papers first appeared, stated that inducements were even held out to him by the English ministry. It is difficult to know upon what basis this statement rests. Thomson, the author of the *History of Chemistry*, in mentioning the circumstance expresses his doubts as to its truth, and states that he made inquiries of Sir Joseph Banks, Cavendish, and Kirwan, but none of them had ever heard of the matter. Indeed, it is intrinsically improbable. "I am utterly at a loss," says Thomson, "to conceive what one individual in any of the ministries

of George III. was either acquainted with the science of chemistry or at all interested in its progress. . . . If any such project ever existed, it must have been an idea which struck some man of science that such a proposal to a man of Scheele's eminence would redound to the credit of the country. But that such a project should have been broached by a British ministry, or by any man of great political influence, is an opinion that no person would adopt who has paid any attention to the history of Great Britain since the Revolution to the present time." However this may be, there is one name that suggests itself as the possible author of such a project, and that is Lord Shelburne. Had Thomson been able to question Priestley on the subject, the real ground for Crell's statement might have been elicited.

But Scheele's love of quiet and retirement was too deep-seated to allow him to exchange Köping for a foreign capital. Even if he should be forced to leave the little town, Lokk was ready to take him back to Upsala. His yearning for independence and for the tranquil life which Köping had seemed to promise held him there. "One needs not to eat more than enough," he wrote to Bergmann, "and if I can find my bread in Köping, there is no occasion to seek it elsewhere." Other influences, too, were at work. The burghers of the place and the gentry of the neighbourhood combined to induce him to remain. The former, mindful, as they said, of the reproach that in parting with Scheele they would be neglectful of the benefit, no less than of the honour, to the town, declared their intention of dealing with no other apothecary; whilst the latter, headed by the principal man of the province, expressed their willingness to move for a new privilege, so as to enable him to start an independent business. This remarkable exhibition of

popular sympathy at length compelled the Sonnemans to accept the young *provisor*, and Scheele was duly installed at Köping. But herein fortune showed herself even less kind than is her wont. Scheele, after all, had gathered Dead Sea fruit. Instead of the prosperous, well-ordered business he had been led to expect, he found little but debts and discomfort. Such a blow would have crushed a weaker man. He accepted his lot uncomplainingly; we search in vain amongst the letters for a word of railing or accusation. Scheele, in truth, had been schooled in adversity, and many a hard and bitter lesson had taught him how to grapple with it. Patiently, and with a tenacity of purpose which is wellnigh sublime in its heroic self-abnegation, he deliberately set himself to retrieve the fallen fortunes of the widow's estate.

For years his life was a continual struggle with privation, relieved to some extent by an annual grant of 100 rix-thalers, which the Academy, at Bergmann's instigation, made him in 1777. In the previous year he acquired full possession of the pharmacy, and the last of the widow's debts was at length paid. The tide had now turned. In 1782 his circumstances had so far improved that he was able to build himself a new house, with a good and well-furnished laboratory. If not rich he had at least a sufficiency; a modest competency was, indeed, all he desired, for Scheele was one of those men whose riches consist, not in the abundance of their possessions, but in the fewness of their personal wants. He was now in the prime of life, and in the full maturity of his mental vigour. His scientific position was assured, and his name was mentioned with honour and respect in every intellectual centre in Europe. Many years of scientific activity were, in all human probability, before him.

Although never of robust health, he had been fairly free from illness up to his thirty-fifth year, when he contracted rheumatism from working, in the rigour of a Scandinavian winter, in the outhouse which at that time did duty as his laboratory. During the autumn of 1785 he suffered greatly, not only from rheumatism, "the natural heritage," he says, "of all apothecaries," but from a weariness and dejection even harder to bear. He still worked on, however. In the early part of 1786 he sent a memoir to the Academy on gallic acid. In the March of the same year he was studying the action of light on nitric acid. "I will repeat the experiments," he wrote, "during the coming summer. We shall then see what will come of them." That summer never came to Scheele. The rheumatism brought other disorders in its train, and he instinctively felt that his end was near. Some time before his fatal illness he had formed the resolution of marrying the widow Pohl, who, together with his sister, who died in 1780, had kept house for him at Köping. On his deathbed he carried out this project, that he might leave to her once more the business he had striven so manfully to preserve. Two days afterwards—21st May 1786—he died, in the forty-third year of his age. The brave man who had struggled with such unflinching courage in the storms of fate had conquered but to die. A new *provisor* quickly appeared, and within a few months the widow was again a wife.

The true history of Scheele's life is, after all, to be found in his works. "What we call a genius," said Pope, "is hard to be distinguished by a man himself from a strong inclination." Scheele himself would have been the first to admit that his strongest inclination was to experiment, and the rest of the world has said that herein lay his genius. His old master, Kjellström, has

recorded that such phrases as "Das kann sein"; "Das ist nicht richtig"; "Das werde ich untersuchen," were ever on his lips as he pored over the chemical literature of his time. This incessant mental activity was fruitful in investigations in every department of chemistry. We owe to Scheele our first knowledge of chlorine and of the individuality of manganese and baryta. He was an independent discoverer of oxygen, ammonia, and hydrochloric acid gas. He discovered also hydrofluoric, nitro-sulphonic, molybdic, tungstic, and arsenic acids among the inorganic acids; and lactic, gallic, pyrogallic, oxalic, citric, tartaric, malic, mucic, and uric among the organic acids. He isolated glycerin and milk-sugar; determined the nature of microcosmic salt, borax, and Prussian blue, and prepared hydrocyanic acid. He demonstrated that plumbago is nothing but carbon associated with more or less iron, and that the black powder left on solution of cast iron in mineral acids is essentially the same substance. He ascertained the chemical nature of sulphuretted hydrogen, discovered arsenetted hydrogen, and the green arsenical pigment which is associated with his name. He invented new processes for preparing ether, powder of algaroth, phosphorus, calomel, and *magnesia alba*. His services to quantitative chemistry included the discovery of ferrous ammonium sulphate, and of the methods still in use for the analytical separation of iron and manganese, and for the decomposition of mineral silicates by fusion with alkaline carbonates.

To Scheele, however, the greatest work of his life was his memoir on *Air and Fire*, which appeared in 1777, and which, on account of its relations to the chemical theory of that time, attracted universal attention, and was translated into almost every European

language. The chief part of the experimental material for this work, as is proved by the correspondence and laboratory memoranda now published, was collected partly in Malmö and Stockholm—that is, before the autumn of 1770, and partly during the earlier portion of his stay in Upsala—that is, prior to 1773. These dates are important in view of Scheele's relations as a discoverer to Priestley and Lavoisier. A number of circumstances, and more especially the dilatoriness of the publisher Swederus, retarded the appearance of the book. From the letters to Gahn it appears that the manuscript was sent to the printer towards the close of 1775, but nearly two years elapsed before the work was made public. Scheele, in several of his letters, complains bitterly of the delay. In August 1776 he wrote to Bergmann: "I have thought for some time back, and I am now more than ever convinced, that the greater number of my laborious experiments on fire will be repeated, possibly in a somewhat different manner, by others, and that their work will be published sooner than my own, which is concerned also with air. It will then be said that my experiments are taken, it may be in a slightly altered form, from their writings. I have to thank Swederus for all this." No imputation of plagiarism was ever brought against Scheele. The whole conduct of his life was proof indeed against even a suspicion of unfair dealing. Although on occasions he could show that he had the *mens sibi conscia recti*, and could manifest a proper assurance in his own vindication, he was singularly unselfish and unworldly. With all Priestley's candour and sense of rectitude, he had Cavendish's indifference to fame and his contempt for notoriety. It can hardly be doubted, however, that had Scheele's work appeared in 1775 he himself would

have occupied a still higher position in the estimation of his contemporaries, and that it would not have been left to posterity to assign him his true place in the history of scientific discovery.

It is impossible to read this, or indeed any other of Scheele's memoirs, without being impressed by his extraordinary insight, which at times amounted almost to divination, and by the way in which he instinctively seizes on what is essential and steers his way among the rocks and shoals of contradictory and conflicting observations. No man was more staunchly loyal to the facts of his experiments, however strongly these might tell against an antecedent or congenial hypothesis. "Es ist ja nur die Wahrheit," he wrote to Hjelm, "welche wir wissen wollen, und welch ein herrliches Gefühl ist es nicht, sie erforscht zu haben." Had these facts been worked out by their discoverer in the spirit of quantitative accuracy so characteristic of his contemporary Cavendish, they would inevitably have undermined phlogistonism, even if they would not have effected its overthrow, before the advent of Lavoisier. As it was, other heads and other hands made use of them to demolish the theory by which their author could alone explain them, and to which he vainly imagined they lent so strong a support. It is, perhaps, idle to speculate on the causes which prevented Scheele from recognising the full significance of his work. It may be that from the lack of mathematical training the quantitative aspects of chemistry had few attractions for him, but it is equally probable that the peculiar character of his inquiries may have been determined by the circumstances of his position, by his poverty, and by the want of the refined and costly apparatus needed for quantitative research. But surmises, as Scheele himself said, cannot

determine anything with certainty. It must be admitted that he was wanting in the faculty of co-ordination, grasp of principle, and power of generalisation, that so strikingly characterise Lavoisier; and his greatest investigation, whilst it testifies to his genius as an experimentalist, reveals, no less clearly, his weakness as a theorist. But when every legitimate deduction has been made, Scheele's work, with all its shortcomings and limitations, stamps him as the greatest chemical discoverer of his age. His story constitutes, indeed, one of the most striking examples of what may be achieved by the diligent cultivation of a single natural gift.

IV

HENRY CAVENDISH

A Lecture delivered in the Hulme Town Hall, Manchester, on 24th November 1875. Manchester Science Lectures

When I had the honour to appear here on a former occasion I gave you some account of the life and labours of a famous Yorkshire philosopher, Joseph Priestley, one of the most illustrious of that remarkable band of learned men which did so much to make the reign of George III. what Lord Brougham was wont to declare it to be —the Augustan age of modern history.

To-night I shall venture to offer you a brief notice of the character and work of another and equally illustrious member of that band—Henry Cavendish. These two men had, however, little in common beyond their zeal for science ; indeed, it is scarcely possible to conceive of a stronger contrast than that which their personal histories afford. Priestley, the son of a poor cloth-dresser, was ardent, impulsive, ingenuous—fond of the strife of words, never so happy, indeed, as when, Ishmael-like, his hand was against everybody and everybody's hand was against him. Cavendish, a scion of a great house, was cold, retiring, reticent, passively selfish, a confirmed misogynist, a hater of noise and bustle. It was said of him that he probably uttered fewer words

in the course of his fourscore years than any man who ever lived so long—not even excepting the monks of La Trappe. Priestley delighted in literary composition; his pen was ever busy; he published more than a hundred works on subjects of the most extraordinary diversity, turning them off with an ease and rapidity which even the most prolific of lady novelists might envy. Cavendish, although he wrote much, printed fewer pages than Priestley did books; his morbid shyness, and his horror of publicity, compelled him to keep back his scientific memoirs even when he had prepared them for publication.

But that you may the better frame for yourselves some conception of the manner of man Cavendish was, let me attempt to sketch for you a scene in which he might have played a part. That there is nothing opposed to truth in it you may readily determine for yourselves, if what I say to-night may so far interest you in Cavendish as to lead you to read his life as written by Dr. Wilson or by Lord Brougham. Imagine, then, you are in the London of ninety years ago: it is night, and you are standing before an old-fashioned house in what is now a very unfashionable square. It is evident from the lights in the windows and the bustle before the door that there is a dinner-party or some social meeting in the house. A couple of chairmen have deposited a portly gentleman, with a large frill, on the step, and two or three lumbering vehicles, having set down their charges, are rattling away over the rough stones into the obscurity of the dimly-lighted street. My knowledge of London ninety years ago is so vague that I must ask you to complete the picture for yourselves by throwing in any other accessories which may occur to you as giving it a strong eighteenth-century flavour,

such as a few link-boys, a solitary watchman, an oil lamp or two, and a plentiful sprinkling of puddles and mud. You are informed that the house belongs to Sir Joseph Banks, who is the President of the Royal Society of London, and that the occasion is one of his weekly conversaziones. The portly visitor, with the large frill, makes his way upstairs, to the evident embarrassment of a thin, middle-aged gentleman in an old-fashioned Court dress of faded violet and a knocker-tailed periwig, who is moving uneasily about on the landing, manifestly afraid to face the assembly. The approach of the gentleman on the stairs, however, drives him into the room. He shuffles quickly from place to place, his manner is awkward ; his face betrays a nervous irritation of mind, and he appears annoyed if looked at. It is the Honourable Mr. Cavendish. Finding himself close to a group, evidently, from the appearance which their faces wear, speaking of a deeply important matter, he draws near to listen. They are talking of a rumour of some grave disaster which has befallen my Lord Cornwallis and his troops, who it would seem have been circumvented in some unexpected manner by the machinations of that arch-rebel Washington. Mr. Cavendish is scarcely interested, and he moves aside to catch something concerning, it may be, some fresh eccentricity of poor Lord George Gordon, or perhaps some account of the troubles of the unhappy Mr. Watt, the engineer, who, it is being said, is fighting tooth and nail to defend his just rights from a set of unprincipled rogues who pirate his inventions. None of these matters is sufficiently moving to detain him. But his manner quickly alters when he overhears the mention of the name of Mr. Herschel. Mr. Herschel is a musician at Bath, who employs his leisure in constructing big telescopes, with one of which

he has just discovered a new planet. Mr. Cavendish is greatly interested; he listens with marked attention; he is even about to put a question, and begins in a nervous, hesitating manner, and in a thin, shrill voice, when his eye catches that of a stranger; he is instantly silent, and retires in great haste, for he has a horror of a strange face. The portly gentleman with the large frill espies him, and comes up with a foreign gentleman, who is formally introduced to Mr. Cavendish. Mr. Cavendish is assured by the portly gentleman that his foreign friend is particularly desirous to make the acquaintance of a philosopher so profound and so universally celebrated—all of which is confirmed by the foreign gentleman, who adds that it was, indeed, his chief reason for coming to London, that he might see and converse with one of the greatest ornaments of Britain, and one of the most illustrious philosophers of that or any other age. Mr. Cavendish is speechless; he is overwhelmed with confusion, until, seeing an opening in the crowd, he darts through it with all possible speed, and, reaching his carriage, is driven home. His house is precisely such as you would expect from one of his habits and disposition; it is made up of laboratories and workshops, and very little is set apart for personal comfort. The principal laboratory is in what the builder intended to be the drawing-room; in an adjoining chamber is a forge; and the upper apartments are turned into an astronomical observatory. Mr. Cavendish rarely did violence to his love of solitude by asking any one to his house. If a friend chanced to dine with him, he was invariably treated to a leg of mutton, and nothing else. We are told that on one occasion, three or four guests being expected, he was asked what was to be got for dinner. He replied with the customary formula, "A

leg of mutton." "But," said the servant, "that will not be enough for five." "Then get two legs," was his answer. During the latter part of his life Mr. Cavendish was immensely rich. At the time of his death he was said to be worth a million and a quarter, and was the largest holder of Bank Stock in England. But he who was said to be the most wealthy of learned men, and the most learned of wealthy men, seemed quite indifferent to his riches. There is a well-known story of his threatening to remove his money out of the hands of his bankers if, as he said, they continued to plague him about it. Cavendish, as you may suppose, could never be induced to sit for his portrait; but an artist, who was bent upon having it, managed to get near his subject unobserved, and first sketching the three-cornered hat, and then the greatcoat, he patiently watched his opportunity and inserted the profile between them. This, I believe, is the only known or authentic portrait of Cavendish.

The life of such a man is, as you may well imagine, nearly devoid of incident. There is but little more of his personal history to tell, except that he was the son of Lord Charles Cavendish, that he was born at Nice in 1731, and that he died in London in 1810. He died as he had lived, voluntarily severing every tie of human sympathy. When he found himself near his end, he called his servant to his bedside, and said, "Mind what I say—I am going to die. When I am dead, but not till then, go to Lord George Cavendish and tell him—Go!" The dying man wished to be alone, and the servant, who hesitated to leave him, was ordered from the room. In half an hour he returned to find that his master had turned his face to the wall, and quietly passed away.

There is nothing lovable in such a character; on the other hand, there is nothing in it that is despicable. This passionless man, whose moral character seemed almost a blank, had a marvellously clear intelligence, and a range of mental vision second to none of his age. In extent of acquirements, and in profundity of learning, he was unsurpassed by any of his contemporaries. His published work, although of the highest order, gives a very incomplete idea of his powers. He left behind him a mass of papers which indicate that he was far in advance of the science of his time. His memoirs on heat and electricity contain the germs of discoveries, if not actual discoveries, which are commonly associated with the names of subsequent investigators. He was an accomplished practical astronomer and a profound mathematician. His knowledge of the calculus and the manner in which he handled it have been described as masterly.

Science is indebted to a learned Scotch professor of the eighteenth century—Dr. Black—for the discovery of certain fundamental laws of heat; and the elucidation of these laws seems to have been the subject of Cavendish's earliest inquiries. One of the problems he set himself to solve, in the course of these investigations, was whether our mercurial thermometer was an accurate and uniform measurer of temperature, to the extent of showing whether the temperature of a mixture of hot and cold water is the mean of the temperatures of the hot and cold water before mixing. Having found that such was the case, Cavendish proceeded to determine the effect of mixing dissimilar liquids at different temperatures. "One would naturally imagine," he says, "that if cold mercury, or any other substance, is added to hot water, the heat of the mixture would be the same as if an equal

quantity of water of the same degree of heat had been added, or, in other words, that all bodies heat and cool each other when mixed together equally in proportion to their weights."

He then shows by experiment that such is not the case. He mixed quicksilver and water together at different temperatures, and found that if it required 1 lb. of water at a known temperature to cool a certain weight of hot water through a certain number of degrees, it would require 30 lbs. of quicksilver to cool the same weight of hot water through the same interval of temperature. He made trials with various metals, with sulphur, glass, charcoal, and many other bodies, and he concludes "that the true explanation of these phenomena seems to be that it requires a greater quantity of heat to raise the heat of some bodies a given number of degrees by the thermometer than it does to raise other bodies the same number of degrees."

We have here the first clear enunciation of a very important matter: if Cavendish had communicated his discovery to the world when he made it, namely in 1764, he would have had priority over those who are generally styled the discoverers of the fact of specific heat.

Cavendish did much to improve the mercurial thermometer. He pointed out several sources of error in the methods of making and using it. He was the first to insist on the necessity of correcting its indications when the whole of the mercury is not within the space of which the temperature is to be ascertained, and the first to draw up special directions to ensure uniformity in the mode of graduating it. He also accurately determined the temperature at which quicksilver freezes, and found it to be 39 degrees below the point at which water

is ordinarily turned into ice. But it would require an entire evening to tell you all that Cavendish did on the subject of heat. That it occupied much of his attention is obvious from the number and character of his experiments, and the excellence of his numerical results. It is evident, too, that he thought deeply on the nature of heat. He rejected the doctrine that it was material, rather holding, as he tells us, "Sir Isaac Newton's opinion, that heat consists in the internal motions of the particles of bodies"; the theory in fact which is now, I should suppose, universally current. And it is worthy of remark that one of the greatest exponents of this theory was the director of one of the finest physical laboratories in the world—a laboratory erected at Cambridge to the memory of Cavendish by his descendant, the late Duke of Devonshire.[1]

Cavendish was a natural philosopher in the widest sense of the term, for he occupied himself in turn with every branch of physical science known in his time. But it is to his discoveries in chemistry that his fame is chiefly due; and here again we may trace the influence of Black in directing the current of his early inquiries. Chemists, up to the middle of the eighteenth century, had no clear conception of the existence of a variety of gaseous substances perfectly distinct from one another. They were inclined to believe that all the different forms of gas they met with were merely modifications of one and the same substance. Their distinctive characters were supposed to arise from their being "tainted," or "infected with fumes, vapours, or sulphurous spirits." The publication of a celebrated essay by Black on "Magnesia Alba" marked an epoch in the history of chemistry by demonstrating the existence of at least one gaseous

[1] The late Professor Clerk Maxwell.

body totally distinct from the air we breathe. Black showed that the difference between chalk and quicklime was due to the presence of a gas in the chalk which was not in the quicklime. Quicklime, indeed, had the property of fixing this air, and of thus being converted into chalk. Black named this air, which was so capable of entering into the composition of bodies, "fixed air"; nowadays we call it carbon dioxide, a name which denotes its composition, of which Black was ignorant. Black did very little towards investigating this gas in the free state. The first full account of its properties was given by Cavendish in 1766. Cavendish prepared the fixed air with which he experimented by dissolving marble, which is, chemically speaking, the same thing as chalk, in spirits of salt, or hydrochloric acid. He found that the gas dissolved in its own bulk of water at common temperatures, and that cold water dissolves more of it than hot water; indeed, he says, "water heated to the boiling point is so far from absorbing the air that it parts with what it had already absorbed." Lime and alkalis, especially if dissolved in water, rapidly absorb the gas, but it may be collected and preserved over quicksilver for any length of time; indeed chemists owe the idea of using quicksilver to collect and preserve certain gases which are absorbed by water to Mr. Cavendish.

Although you are blessed here in Manchester with one of the best water supplies in the kingdom, you doubtless have heard of what are called "hard" waters; you may even know that some of these hard waters are made "soft" by boiling, and that the kind of hard water which is softened by boiling deposits a crust or "fur" in the tea-kettle, and a "cake" in the steam-boiler. Now this "fur" is mainly composed of chalk,

kept in solution in the water by the fixed air dissolved therein. When the water is boiled the fixed air is expelled, as Cavendish tells us, and accordingly the chalk is deposited. This explanation of the origin of the "fur" was first given by Cavendish. Possibly some of you may know that such hard waters are frequently softened on the large scale by adding lime to them. The lime combines with the fixed air (the agent, you bear in mind, which keeps the chalk in solution), and accordingly the chalk is deposited, together with that formed by the union of the fixed air with the added lime. The fact that water could be thus deprived of its dissolved chalk was pointed out by Cavendish. When the carbon dioxide is allowed gradually to escape from the solution, the carbonate of lime is deposited in small crystals, the shapes of which are often exceedingly curious and beautiful; indeed, there is no substance which has such a diversity of crystalline form as this carbonate of lime.

In various parts of the world, particularly in districts where limestone abounds, there are large caves, or grottoes, from the roofs of which depend long icicle-shaped masses of carbonate of lime termed *stalactites*. If you notice one of these masses you will observe that occasionally a drop of water falls from the end of it to the floor, or rather upon a similar mass of carbonate of lime on the floor, exactly underneath that which hangs from the roof. The lower mass, which appears to stretch up towards the upper one, is termed a *stalagmite*. Occasionally the two masses meet one another and unite to form a continuous column. The origin of these masses—these stalactites and stalagmites—will readily occur to you: the rain-water percolating through the rock above the cave contains carbonic acid in solution,

by which it dissolves the carbonate of lime in the rock. As it drips from the roof it gives up a portion of its carbonic acid to the air in the cavern, and accordingly a portion of the carbonate of lime is deposited; the next drop runs over the mass so deposited, and by giving out another portion of dissolved carbonic acid deposits another portion of carbonate of lime on the first deposition; and so the process goes on, each portion of water from the roof running down the icicle of carbonate of lime which is formed, and continually adding to its length. But the drops fall off to the floor long before they have given up the whole of their carbonic acid, and therefore long before they have yielded up all the chalk which they held in solution. Accordingly the escape of the carbonic acid goes on from the water after it has fallen on the floor, and so you get this second deposit of carbonate of lime—this stalagmite—formed underneath the stalactite.

Cavendish also showed that fixed air was considerably heavier than common air by weighing a bladder filled first with the one gas and then with the other. The fixed air he found to be one and a half times heavier than the common air.

The old chemists, who in days gone by greatly busied themselves to discover a more direct method of turning things into gold than is practised by their successors in the chemical arts, have left us some marvellous stories concerning the behaviour of a gas which seems to be evolved from certain metals when they are brought into contact with acids, such as oil of vitriol, or muriatic acid. The exact nature of this gas remained unknown until Cavendish investigated its properties. This gas, which we now call hydrogen, is highly inflammable, and Cavendish showed that, like many other inflammable

bodies, it cannot burn without the assistance of common air. When mixed with rather more than double its volume of air, it explodes violently on the approach of a light. He also weighed this gas by the same method which he had employed to weigh the fixed air, and he found it to be eleven times lighter than common air. Cavendish, however, underestimated the lightness of this gas; in reality it is about fourteen and a half times lighter than air.

When giving you an account of Priestley's work, I described to you his method of analysing the air. It was based on the fact that when the gas known as *nitric oxide* comes in contact with air, the oxygen in the air combines with the nitric oxide to form a product soluble in water. If the mixture of gases is made in a tube standing over water, the diminution in volume, consequent on the removal of the oxygen, is a measure of the amount of that gas in the air. As the quality of the air was supposed to depend upon the diminution of volume which it suffered by being mixed with nitric oxide, the instruments designed to make the tests were termed *eudiometers*, from two Greek words denoting a "measure of goodness." Without going into details, I may say that this method of analysis is liable to an objection from the cause first worked out by our illustrious townsman, John Dalton, that the same volume of oxygen can combine with different volumes of the nitric oxide. This fact was indeed known to Cavendish, and he made a great number of experiments in order to ascertain the best method of mixing the gases so as to obtain constant results.

By means of the apparatus he devised he was enabled to show that the composition of the atmosphere is sensibly constant. He tells us that "during the last

half of the year 1781 I tried the air of near sixty different days . . . but found no difference that I could be sure of, though the wind and weather on those days were very various, some of them being very fair and clear, others very wet, and others very foggy." This conclusion is in harmony with the results of later experimenters. The atmosphere has practically the same composition all the world over, and all the year round. Although there are slight variations in the relative proportion of the constituents, methods of the highest precision are required in order to detect them. Cavendish gives us the numerical results of his experiments, and from these it appears that, when expressed in the manner we now adopt, the mean composition of the air is in 100 parts by measure :—

Oxygen	20·8
Nitrogen	79·2

The most refined analytical methods of modern times have shown that the average numbers are

Oxygen	20·9
Nitrogen	79·1

A result, you see, almost identical with that deduced from Cavendish's observations, and one which illustrates in a very striking manner the extreme care and accuracy with which he worked.

Cavendish next proceeded to determine the cause of the diminution in volume which common air occasionally suffers when substances are caused to burn in it.

Among the many experiments which he made in order to elucidate this matter there is one which is especially remarkable, as it led him to his greatest discovery, that of the composition of water—a discovery which will make the name of Cavendish for ever memorable.

Dr. Priestley relates in one of his volumes of *Experiments and Observations on Air*, that when a mixture of common air and inflammable air is exploded by the electric spark in a glass vessel, "the inside of the glass, though clear and dry before, immediately became dewy." "As this experiment," says Cavendish, "seemed likely to throw great light on the subject I had in view, I thought it well worth examining more closely." Cavendish repeated this experiment in his characteristically careful manner. The inflammable air and common air were mixed in varying but known proportions, and the diminution in volume which attended the explosion was accurately noted in each case, and the amount of oxygen remaining in the air was determined by the eudiometer. Cavendish found that the greatest diminution of volume occurred when two volumes of hydrogen were mixed with five volumes of air.

He tells us that when this mixture is exploded, "almost all the inflammable air and about one-fifth part of the common air lose their elasticity, and are condensed into the dew which lines the glass." Cavendish continues: "The better to examine the nature of this dew 500,000 grain measures of inflammable air were burnt with about two and a half times that quantity of common air, and the burnt air made to pass through a glass cylinder 8 feet long and $\frac{3}{4}$ inch in. diameter, in order to deposit the dew. The two airs were conveyed slowly into this cylinder by separate copper pipes, passing through a brass plate which stopped up the end of the cylinder; and as neither inflammable air nor common air can burn by themselves, there was no danger of the flame spreading into the magazines from which they were conveyed. . . . By this means upwards of 135 grains of water were condensed in the cylinder,

which had no taste nor smell, and which left no sensible sediment when evaporated to dryness; in short, it seemed pure water. . . . By the experiments with the globe it appeared that when inflammable air and common air are exploded in a proper proportion, almost all the inflammable air and near one-fifth of the common air lose their elasticity, and are condensed into dew. And by this experiment it appears that this dew is plain water, and consequently that almost all the inflammable air and about one-fifth of the common air are turned into pure water."

Cavendish then repeated the experiment with pure oxygen, or "dephlogisticated air," as this gas was then termed. I will give you the result in his own words, for the account has a great historical interest: "I took a glass globe holding 8800 grain measures, furnished with a brass cock, and an apparatus for firing air by electricity. This globe was exhausted by an air-pump, and then filled with a mixture of inflammable and dephlogisticated air by shutting the cock, fastening a bent glass tube to its mouth, and letting up the end of it into a glass jar, inverted in water, and containing a mixture of 19,500 grain measures of dephlogisticated air, and 37,000 of inflammable; so that on opening the cock some of this mixed air rushed through the bent tube and filled the globe. The cock was then shut, and the included air fired by electricity, by which means almost all of it lost its elasticity. The cock was then again opened, so as to let in more of the same air, to supply the place of that destroyed by the explosion, which was again fired, and the operation continued till almost the whole of the mixture was let into the globe and exploded. By this means, though the globe held not more than the sixth part of the mixture, almost the whole of it was exploded

in it, without any fresh exhaustion of the globe." Cavendish, however, found that in many of his trials the condensed water was sensibly acid to the taste, and by saturation with alkali, and evaporation, it yielded nitre. The search for the cause of the formation of this acid led Cavendish to another discovery—namely, that of the composition of nitric acid, an acid which is probably familiar to you under its old name of spirits of nitre or *aqua fortis*. He showed that the formation of this acid was not an essential part of the process of the union of the oxygen and hydrogen, but that it was due to the presence of impurities in the gases used. Whenever the amount of oxygen was larger than could combine with all the hydrogen in the mixture, a portion of that oxygen united with the nitrogen of the common air present, and so formed the nitric acid.

Such, then, were the experiments which led to the discovery, firstly, of the compound nature of water; secondly, of the character of its constituents; and thirdly, of the proportions in which these constituents are combined together. It would be impossible to overestimate the value of this discovery: it marks one of the great epochs in the history of chemistry. Who could have predicted that this most familiar of all liquids —a liquid, too, long regarded as the very type of a chemical element—was composed of two colourless invisible gases—the one the inflammable hydrogen, the lightest substance known—the other, oxygen, the life-sustaining principle in the air we breathe—nay, the element which has been styled "the chemical centre in the scheme of nature"?

Nearly every important discovery has to pass through two ordeals—it is first impugned as not true, and then as not new; and this great discovery which I have

ascribed to Cavendish formed no exception to this rule. Not many years ago there was a great controversy concerning the question—Who was the discoverer of the composition of water? I am not now going to rake up the matter, for it is gradually being forgotten; but I think that every chemist now allows that the claims of Cavendish have been incontestably proved. The fact is the time was ripe for this discovery. Everybody familiar with the chemical work of the latter half of the eighteenth century will admit that the labours of a dozen of Cavendish's contemporaries were tending more or less directly to the same goal, and had Cavendish proved unequal to his opportunities, his grandest discovery would not have been long delayed. It has been said that the discovery of law is regulated by law, and the history of the discovery of the composition of water affords a striking exemplification of the truth of this remark.

The time will scarcely allow me to tell you more of what Cavendish did; but, if I am not trespassing too much on your patience, I should like just to mention another great work of his, since any account of Cavendish's labours would be very incomplete without some reference to it. An ancestor of Cavendish's was one of the first to sail round the earth. Cavendish himself was one of the first to attempt to weigh it. Cavendish, in fact, undertook to determine how much heavier the earth is than a spheroid of water of equal size. The apparatus which he employed consisted of a long light wooden rod suspended horizontally by a thin wire. At the ends of the rod were leaden balls about two inches in diameter, and near these could be brought two large spherical masses of metal. By the mutual attraction of the balls, big and little, the long rod was caused to move slightly. The amount of the deviation, and the force

necessary to produce it, being known, together with the weight of the balls, and the distances from their centres, the attraction of a spheroid of water of the same diameter as the earth upon the ball on its surface can be calculated, from which can also be calculated the relation of the earth's density to that of water. From his experiments, Cavendish concluded that the earth is about five and a half times heavier than water—a result which the subsequent labours of Mr. Baily, made with extraordinary care and patience, have shown to be very near the truth. It deserves to be mentioned, however, that Newton, with that marvellous insight which nowadays seems to us nothing less than divination, had predicted that the earth would be found to be between five and six times heavier than water.

One more remark and I have done. A celebrated French chemist, whose patriotism we admire scarcely less than his genius, has declared that "Chemistry is a French Science, its founder was Lavoisier, of immortal memory." The merit of Lavoisier is undoubtedly great, and the influence which he exerted on the development of chemistry was profound. It is accounted the chief glory of Lavoisier that he first clearly pointed out that the principles of gravitation lie at the basis of chemistry; that chemistry is in fact a science of quantitative relations. But let us take a retrospect of Cavendish's labours. He fixed the weight of the earth; he established the proportions of the constituents of the air; he occupied himself with the quantitative study of the laws of heat; and lastly, he demonstrated the nature of water and determined its volumetric composition. Earth, air, fire, and water—each and all came within the range of his observations. Now, I ask you, what is the most obvious characteristic of all this labour? Is it not its

thoroughly quantitative character? Weighing, measuring, calculating; such, indeed, was pre-eminently the essential nature of Cavendish's work. If, then, the claim of any one to be styled the founder of chemistry as a science rests upon his recognition of its quantitative relations, may we not also, and with equal truth, say that "Chemistry is an English Science—its founder was Cavendish, of immortal memory"?

V

JAMES WATT

AND

THE DISCOVERY OF THE COMPOSITION OF WATER

Being the Watt Anniversary Lecture delivered before the Greenock Philosophical Society on 11th March 1898

When your Secretary did me the honour to communicate the wish of the Committee that I should deliver this lecture, he was good enough to send me a list of the names of my predecessors in the position I was invited to occupy, together with a statement of the subjects on which they had addressed you. I confess I read his letter with very mingled feelings. To be asked to form one of such a distinguished company was in itself an honour which I deeply appreciated. On the other hand, it seemed well-nigh hopeless to find any theme associated with the life and work of the great man whose services to humanity we are this day called upon to commemorate that had not been dealt with by one or other of those who had preceded me. Naturally, and as befits the subject, the greater number of those who have spoken on these occasions have been distinguished engineers and mechanicians, and they have been able to speak with a fulness of knowledge and a weight of authority on the outcome of the great engineer's labours to which I,

who know nothing of engineering or machinery, can have no pretensions.

It has occurred to me, however, that there might be one incident in Watt's career which, in all probability, had not been handled by any one of those whom you have invited to appear here, and on which, as it comes within my own province, I thought I might venture, without presumption, to engage your attention. I was the more impelled to select it, in that it illustrates one side of Watt's intellectual activity which those who regard him only as an inventor and a mechanician are apt to undervalue, or even to lose sight of altogether. It serves, too, to throw additional light upon his mental character and his moral worth, and thus enables us to form a fuller and more just appreciation of the attributes of the man we wish to honour. The incident, in a word, relates to Watt's share in the establishment of the true view of the chemical nature of water.

To the historian of science this is doubtless an old story on which it would be difficult to say anything new. The literature concerned with it occupies many volumes, largely owing to the circumstance that it has given rise to a controversy which has engaged the active interest of some of the strongest and subtlest intellects of the nineteenth century. Some of the disputants have been men like Brougham, Jeffrey, and Muirhead, skilled in the arts of advocacy and in the faculty of eliciting and weighing evidence, who have stated their conclusions with all the "pomp and circumstance" of a judicial finding; others are men like Arago, Dumas, Harcourt, Whewell, Peacock, Kopp, George Wilson, eminent in science and literature, who have defended their convictions with great power, ample knowledge,

much argumentative force, and occasional eloquence. At one time the contest was waged with no little fury and bitterness; it threatened, indeed, like the famous controversy as to the proper form of a lightning-conductor, during Sir John Pringle's presidency of the Royal Society, or like the equally famous controversy as to the true discoverer of the planet Neptune, to attain the dignity of a national question, far more acute, I should imagine, than that which has recently occasioned all right-feeling Scotchmen to approach the Queen in Council on the subject of Scotland's proper place and designation in Imperial concerns.

But, happily, the acrimony and ill-feeling have long since passed away. There is no longer any need to discuss the question either as an advocate or as a partisan. What I shall attempt to-night is to treat it dispassionately, and, within the compass of an hour, to assess, as impartially as I am able, Watt's true place in regard to this discovery.

It was indeed an epoch-making event. The discovery of the composition of water was as momentous for science as the greatest of Watt's inventions was for social and economic progress. The very fact itself, apart from all that flowed from it, was of transcendent interest. But to those who had eyes to see, its supreme importance was in its fruitful and far-reaching consequences. It signified nothing less than the passing away of an old order of things: the downfall of a system of philosophy which had outlived its usefulness, in that it no longer served adequately to interpret natural phenomena, and had become rather a hindrance and a stumbling-block to the perception of truth. The discovery at once led to the inception of a more rational and more truly comprehensive

theory, which not only explained what was already known in a fuller, clearer, and more intelligible manner, but pointed the way to new facts hitherto undreamt of,—facts which in their turn served to strengthen and extend the generalisation which led to their discovery. No wonder, therefore, that those who loved and revered Watt, and who were rightly jealous of his honour, should have sought to do all in their power to vindicate what they honestly conceived to be his just title to so signal and so fundamental a discovery.

No man has a juster claim to be regarded as a scientific man, in the truest and noblest sense of that term, than James Watt. The scientific spirit was manifest in him even in boyhood. The very circumstances of his condition, his weakly frame, the solitariness of his school-life, and the early habit of introspection thus induced in a mind forced to feed only on itself, served to strengthen and develop the instinct. Even his early struggles, and the jealousy of the Glasgow Guilds, which forbade him to practise his trade in the burgh in which he had not served an apprenticeship, conduced to mould his character and to determine the bent of his mind. Hard and illiberal as it seemed at the time, the *Zunftgeist* which drove him to the shelter of the old College in the High Street, and secured for him the abiding friendship of Black and Robison, was in reality the most fortunate circumstance of his career. It brought him directly under the influence of one of the greatest natural philosophers of his age, and stamped him permanently as a man of science. It would not be difficult to trace how this influence reacted upon all that Watt subsequently did—from the time of his earliest speculations on the loss of

energy in Newcomen's engine down to the very last of his mechanical pursuits in the dignified retirement of Heathfield Hall. He approached the question of the improvement of the steam-engine as a scientific problem, and under the direct inspiration of the doctrine of the great discoverer of the principle of latent heat. It was this same mental attitude towards scientific truth, the same receptivity for scientific doctrine, the same love of pondering over and speculating upon the true inwardness of things, that brought him the friendship of Priestley, Withering, Wedgwood, and Deluc, and that ultimately made him a cherished member of the foremost scientific academies of the world. It will occasion little surprise to one who has formed a true perception of his character to learn that Watt was wont, even at periods of great mental depression, and of physical suffering, amidst all the toil and anxious worry of a business surrounded with difficulties, to find peace in the contemplation of natural phenomena, and to spend time in philosophical speculation. The shrinking, diffident man, in thus communing with himself and with Nature, followed a true and constant impulse to withdraw from the strife and turmoil of the world, and to seek his pleasure and his rest in the contemplation of natural truth. No one can look upon that contemplative face without being struck with its expression of philosophic calm. What deep, genuine pleasure these communings brought to the harassed man may be gleaned from his correspondence. In truth, Nature intended Watt to be a philosopher of the pattern of Boyle, or Newton, or Dalton: it was destiny that drove him into the world of affairs, where, as he said, he was out of his true sphere. It is necessary to dwell for a moment on this

aspect of Watt in order to form a just appreciation both of his position and of his merits in regard to the great chemical truth with which his name is associated. The man of action is apt to regard the contemplative mind with something akin to contempt. I once heard a bustling, busy man, the head of a large engineering establishment, who had enjoyed the good fortune to be a pupil of Thomas Graham, say of that distinguished philosopher that he was the laziest man he ever met. He did not say he ever knew—for how little he really knew Graham was evident from the fact that at the period to which he referred Graham's thoughts were deeply occupied with some of the most memorable of his investigations.

It was in one of these contemplative moods—in what he himself styled his periods of excessive indolence—and, as it happened, at the very time that the Soho firm was struggling to protect itself against the unprincipled horde that was seeking to infringe Watt's fundamental patent, that he occupied himself with turning over in his mind the outcome of one of his friend Priestley's multitudinous experiments. Watt had long held the view that air was a modification of water, or, as he expressed it in a letter to his friend Black, under date Dec. 13, 1782, that, as steam parts with its latent heat as it acquires sensible heat, or is more compressed, when it arrives at a certain point it will have no latent heat, and may, under proper compression, be an elastic fluid nearly as specifically heavy as water; at which point, he conceived, it would again change its state and become air. As he then relates, he sees a confirmation of this opinion in an experiment of Priestley's, made, as he says, "in his usual way of groping about." "As he [Priestley] had

succeeded in turning the acids into air by heat only, he wanted to try what water would become in like circumstances. He undersaturated some very caustic lime with an ounce of water, and subjected it to a white heat in an earthen retort. . . . No water or moisture came over, but a quantity of air, equal in weight to the water, . . . a very small part of which was fixed air, and the rest of the nature of atmospheric air. . . . He has repeated the experiment with the same result."

About a fortnight later Priestley wrote that he was able to convert water into air "without combining it with lime or anything else, with less than a boiling heat, in the greatest quantity and with the least possible trouble or expense." He added that "the method will surprise more than the effect," but that he would defer "the communication of the hocus pocus of it" until such time as Watt should give him the pleasure of his company in return for the pleasure he was to give Watt in speculating on the subject.

These experiments, as we shall see in due course, were wholly fallacious: in following them up with his wonted ardour Priestley quickly found himself in a maze of contradictions, and ultimately discovered that this seeming conversion was absolutely mythical.

It may be useful, however, to make one or two comments on these passages at the present juncture. In the first place, Watt's opinion as to the relations of water and air, although founded as he thought upon a more philosophical basis, simply embodied the teaching of the schoolmen. The notion that the so-called four elements were mutually convertible, or were in essence identical, ran through the doctrine of twenty centuries of teachers. Despite the onslaughts of the Spagyrists, and of the author of the *Sceptical Chymist*, it per-

meated the literature of natural philosophy down to the very beginning of this epoch. Watt was insensibly swayed by a belief which had descended to him, like the undying germ, through the ages, and he could no more shake himself free of it than he could get rid of the influence of heredity. The very mode in which he, in common with the men of his time, uses the term "air," is an indication of the manner in which this ancient creed limited and cramped his thought. He knew that there were various "airs," but it is very doubtful if he realised that they were essentially different substances. There is abundant evidence in the few chemical papers that he published, and especially in his letters to Black, Priestley, Deluc, Kirwan, and others, that he regarded them all as constituted of the same matter, affected by attributes more or less fortuitous and accidental. Thus, all the varieties of inflammable air were at bottom identical, with properties modified by their origin, or by their varying content of the hypothetical principle phlogiston—that is, the principle that was assumed to make them burn.

From Watt's published correspondence we are able to judge how he regarded Priestley's further work on this so-called conversion of water into air. He admits that the facts are "in some degree contradictory to each other." The apparent conversion would seem to depend upon the material of the vessel in which it was made. In a glass vessel no air was produced, nor was any found in a gun-barrel when the distillation was done slowly, but when confined by a cock, "and let out by puffs it produces much air; which," says Watt, "agrees with my theory and also coincides with what I have observed in steam-engines. In some cases I have seen the tenth of the bulk of the water, of air extricated or made from

it." Davy once said "the human mind is always governed, not by what it knows, but by what it believes, not by what it is capable of attaining, but by what it desires." However willing to catch at anything in support of his belief, it is possible that Watt might have been led to doubt the soundness of Priestley's experiment if an apparent and wholly unlooked-for confirmation of it had not now arisen.

To make the account exact, and in view of what is to follow, it is necessary to go back a little, in point of time. In the spring of 1781 Priestley performed what he styled "a mere random experiment made to entertain a few philosophical friends." It was practically a repetition of Volta's experiment of firing a mixture of the inflammable air from metals, that is, hydrogen, with common air in a closed glass vessel by means of the electric spark. After the deflagration the vessel was found to be hot, and on cooling its sides were observed to be bedewed. Neither Priestley nor any one of his philosophical friends seems to have paid any particular attention to the deposit of moisture, or at all events if they did they failed to perceive its significance. One of them, however, Mr. John Warltire, a Lecturer in Natural Philosophy in Birmingham, imagined that the experiment might afford the means of showing whether heat was ponderable or not, and accordingly he repeated it, using for greater safety a copper globe, weighed before and after the passage of the spark. A minute loss of weight was always noticed, "but not constantly the same: upon the average it was about two grains." [1]

Priestley, who, with Withering, was present when

[1] The account of these experiments is given in a letter to Priestley, and constitutes No. V. of the Appendix to Priestley's *Experiments and Observations* relating to various branches of Natural Philosophy, etc. Vol. II., Birmingham, 1781.

the experiments were made, confirmed the apparent loss of weight; but he adds, with a caution that was not habitual, that he did not think "that so very bold an opinion as that of the latent heat of bodies contributing to their weight should be received without more experiments, and made upon a still larger scale."

Priestley's volume—the sixth in the series—was published in 1781, and was certainly known to Watt; indeed, in the Appendix are printed a number of observations made by him, apparently as the work was passing through the press. Although, therefore, he must have had his attention drawn at about this time to the formation of the dew in Priestley and Warltire's experiment, there is nothing to show that he attached any importance to the circumstance, or that if he did he dissented from Warltire's conclusion that common air deposits its moisture when it is phlogisticated.

For some time previous to the publication of Priestley's book, Mr. Cavendish was engaged upon an inquiry "to find out the cause of the diminution which common air is well known to suffer by all the various ways in which it is phlogisticated, and to discover what becomes of the air thus lost or condensed." In other words, it was an investigation to determine the changes experienced by air when bodies were made to burn in confined portions of it. On the appearance of Priestley's book he repeated Warltire's experiment, thinking "it worth while to examine more closely" as it "seemed likely to throw great light on the subject I had in view." He confirmed the observation on the formation of dew, but although he made the experiment on a large scale, and with varying proportions of the two airs, he was unable to satisfy himself as to the loss of weight after the explosion. As the result of a number of trials made both with the

inflammable air from zinc and from iron, that is, hydrogen, and mixed with common air in the proportion of 423 measures of the inflammable air to 1000 of common air, he says, "we may safely conclude that when they are mixed in this proportion and exploded, almost all the inflammable air and about one-fifth part of the common air lose their elasticity and are condensed into the dew which lines the glass." In order to examine the nature of this dew, large quantities of the hydrogen were burnt with two and a half times its volume of common air, and the product of the combustion was caused to pass through a long glass tube whereby it was condensed. "By this means 135 grains of water were condensed in the cylinder, which had no taste nor smell, and which left no sensible sediment when evaporated to dryness; neither did it yield any pungent smell during the evaporation; in short, it seemed pure water. . . . By the experiments with the globe it appeared that when the inflammable and common air are exploded in a proper proportion, almost all the inflammable air and near one-fifth of the common air lose their elasticity, and are condensed into dew. And by this experiment it appears that this dew is plain water, and, consequently, that almost all the inflammable air and about one-fifth of the common air are turned into pure water."

The idea that common air was for the most part a mixture of two gases—oxygen, or the dephlogisticated air of Scheele and Priestley, and nitrogen, or the mephitic air of Rutherford, the azote of Lavoisier—was familiar to chemists at this period as the result of the teaching of Scheele and Lavoisier; and there is reason to suppose that this opinion was shared by Cavendish. He had been engaged for some time past in an elaborate inquiry into the constitution of atmospheric air, the

results of which admitted of no other interpretation than that common air was composed of two different gases, mixed or combined in constant relative proportions. It is true that in the memoir containing the results of his inquiry he nowhere directly gives his estimate of these relative quantities, but from the data he affords it is easy to deduce both the amount and the constancy of the proportion. Cavendish's papers are characterised by a remarkable conciseness and brevity; an experiment which must have involved the putting together of elaborate and complicated apparatus, and which must have occupied considerable time in its performance, is described in a few lines, and hence it is not always possible to gather with certainty the precise disposition of the arrangements. He never sets out his reasons or his conclusions with any great amount of detail, and his published works occasionally give little indication of his line of thought. But that he clearly recognised that only one portion of common air was concerned in the formation of water, and that this portion was the dephlogisticated air, or oxygen, is obvious from his next series of experiments, in which he fired a mixture of about two measures of hydrogen and one measure of oxygen in a previously exhausted glass globe, furnished with an apparatus for firing air by electricity. When the included air was fired, almost all of it lost its elasticity, so that fresh quantities of the explosive mixture could be introduced and the process repeated until a sufficient quantity of the moisture was obtained for examination. In these experiments Cavendish clearly and definitely demonstrated that the weight of the water was practically equal to the weight of the mixed gases which had combined to form it. In some cases the water was perfectly neutral in its reaction; in

others it was slightly acid, and the cause of this acidity cost Cavendish much experimenting to discover, but he is never in any doubt as to the main result; he says distinctly, "if those airs could be obtained perfectly pure, the whole would be condensed." Now, if Cavendish had published this main result at the time he obtained it, namely, in the summer of 1781, or even if he had formally communicated it to one of the meetings of the Royal Society during the ensuing session, there would have been no Water Controversy. But even if he were ready, it was characteristic of him to delay, not from inertia or indolence, but from a morbid shyness, an unconquerable reticence, which constantly led him to postpone any public announcement of his work. He had the additional, and to him all-sufficient reason, that he had not yet worked out the cause of the occasional acidity of the water. What he did, however, was to communicate the facts of his experiments to Priestley, as Priestley himself states in a subsequent paper published in the *Philosophical Transactions* for 1783. When or how he communicated them to Priestley does not appear, nor have we any means of knowing precisely what was said. Something, however, on this point may be inferred from what Priestley proceeded to do. It appears from a letter to Wedgwood that he repeated Cavendish's experiments during the March of 1783. It will be remembered that he was at this period engaged on his experiments on the seeming conversion of water into air. He had obtained a number of contradictory results, which had led Wedgwood, as far back as the previous January, to put certain sagacious queries which doubtless in the end had their effect in opening Priestley's eyes to the origin of his mistake. But at the time both he and Watt were seeking for fresh evidence

to substantiate the possibility of this conversion. Now just as Cavendish thought that Warltire's experiment might throw light upon the particular matter on which he was then engaged, so Priestley considered that Cavendish's work might afford evidence—indirect, it is true, but still evidence—of the intimate connection between water and air. Cavendish had, he thought, established the converse of the proposition which he and Watt were seeking to prove, in showing that "air," or rather certain kinds of "air," could be converted into water, weight for weight. It was no longer the original Warltire experiment of exploding common air and hydrogen. Cavendish had indicated the particular kinds which were really concerned in the phenomenon, and it was the Cavendish experiment, pure and simple, that Priestley proceeded to repeat. This is obvious from what he says: "Still hearing of many objections to the conversion of water into air, I now gave particular attention to an experiment of Mr. Cavendish's concerning the *reconversion* of air into water by *decomposing* it in conjunction with inflammable air." Priestley here uses the word "decomposing" in a sense contrary to that which the context implies, but that he is consistent in so using it is evident from what follows, and also from similar expressions to be found in his correspondence. But although he professed to repeat Cavendish's experiment, he neglected to do so in Cavendish's manner. He says: "In order to be sure that the water I might *find in the air was really a constituent part of it*, and not what it might have imbibed after its formation [*i.e.* by contact with the water of the pneumatic trough], I made a quantity of both dephlogisticated and inflammable air, in such a manner as that neither of them should ever come into contact with water, receiving

them as they were produced in mercury; the former from nitre, and in the middle of the process (long after the water of crystallisation was come over), and the latter from perfectly made charcoal. The two kinds of air thus produced I decomposed by firing them together by the electric explosion, and found a manifest deposition of water, and to appearance in the same quantity as if both the kinds of air had been previously confined by water. In order to judge more accurately of the quantity of water so deposited, and to compare it with the weight of the air decomposed, I carefully weighed a piece of filtering-paper, and then having wiped with it all the inside of the glass vessel in which the air had been decomposed, weighed it again, and I always found, as nearly as I could judge, the weight of the decomposed air in the moisture acquired by the paper. . . . I wished, however, to have had a nicer balance for the purpose: the result was such as to afford a strong presumption that the air was reconverted into water, and *therefore that the origin of it had been air.*"

These passages, when compared with the accounts given of his own work by Cavendish, strikingly exemplify the difference in the character of the two experimentalists. It would be difficult to pack a greater number of blunders into a couple of paragraphs than are contained in these sentences. The expressions in italics show that Priestley wholly failed to comprehend the true origin of the water. In his laudable anxiety to free the two gases from extraneous moisture he committed blunder after blunder. His method of obtaining the oxygen was bad; that of procuring the inflammable air was worse. Both the gases must have been highly impure, and it was a physical impossibility that they should have given their aggregate weight in

water, even after making every allowance for Priestley's crude and imperfect method of determining it.

Bad as the experimental work was, what it appeared to teach was not lost on Watt: it clearly proved to him that water and air were mutually convertible. How the theory took shape in his mind is evident from the terms in which the two series of Priestley's experiments are coupled together in his letters to Gilbert Hamilton, to Deluc, and to Black. Each set is regarded as complementary to the other, and both taken together are held to prove that air and water are mutually convertible and are therefore essentially the same. Under date 21st April 1783, he tells Black that "Dr. Priestley has made many more experiments on the conversion of water into air, and I believe I have found out the cause of it; which I have put in the form of a letter to him which will be read at the Royal Society with his paper on the subject." He then proceeds to give Black a summary of the three sets of facts, or supposed facts, on which he bases his generalisation, and he makes use of these significant words: "In the deflagration of the inflammable and dephlogisticated airs, the airs unite with violence, become red hot, and, on cooling, totally disappear. The only fixed matter which remains is *water*; and *water*, *light*, and *heat* are all the products. Are we not then authorised to conclude that water is composed of dephlogisticated and inflammable air, or phlogiston, deprived of part of their latent heat, and that dephlogisticated, or pure air, is composed of water deprived of its phlogiston and united to heat and light; and if light be only a modification of heat, or a component part of phlogiston, then pure air consists of water deprived of its phlogiston and of latent heat."

Very similar turns of expression and trains of reasoning are to be met with in other letters to his friends written at about the same period. In all, it is abundantly clear that, whatever may have been his surmises as to the real nature of water, it was the conception of the mutual convertibility of air and water that was uppermost in his mind. These passages, however, constitute Watt's claim to be regarded as the true and first discoverer of the compound nature of water.

Three days after the letter to the Royal Society was written, or rather dated, there came a bolt from the blue in the form of a letter from Priestley to Watt. "Behold," it said, "with surprise and with indignation the figure of an apparatus that has utterly ruined your beautiful hypothesis, and has rendered some weeks of my labour in working, thinking, and writing almost useless." The doubts of Wedgwood, certainly no mean authority on the properties of baked clay, had, in fact, led Priestley to devise an experiment by which it was proved beyond all doubt that this seeming conversion of water into air was really due to an interchange of steam and air, effected by diffusion through the porous material of the retort. Well might Priestley cry to Deluc, "We are undone!" Watt's faith in the "beautiful hypothesis" was no doubt rudely shaken, but it was not shattered. In his answer to Priestley he denied that it was ruined: "It is not founded," said he, "on so brittle a basis as an earthen retort." Priestley, however, would have none of it; theories with him—always excepting the all-comprehensive one of phlogiston, which was the head and front of his creed, as, indeed, of his subsequent offending—had at no time much value, for, as Marat said of

Lavoisier, he abandoned them as readily as he adopted them, changing his systems as he did his shoes. Indeed, he rather prided himself on this capacity for quick change. "We are, at all ages," he once said, "but too much in haste to *understand*, as we think, the appearances that present themselves to us. If we could content ourselves with the bare knowledge of new facts, and suspend our judgment with respect to their causes, till by their analogy we were led to the discovery of more facts of a similar nature, we should be in a much surer way to the attainment of real knowledge." With a candour all his own, he immediately added: "I do not pretend to be perfectly innocent in this respect myself, but I think I have as little to reproach myself with on this head as most of my brethren; and whenever I have drawn general conclusions too soon, I have been very ready to abandon them. . . . I have also repeatedly cautioned my readers, and I cannot too much inculcate the caution, that they are to consider new *facts* only as discoveries, and mere *deductions* from these facts as of no kind of authority; but to draw all conclusions, and form all hypotheses, for themselves."

Watt's mind was of a very different cast. He did not lightly adopt opinions; his convictions were slowly and deliberately formed, and were retained with a corresponding tenacity. But, all the same, he eventually thought it prudent to withdraw his letter, and three days prior to the reading of Priestley's paper which accompanied it, Priestley informed Sir Joseph Banks of Watt's desire that the letter should not be publicly read. That it was withdrawn on account of what Watt calls Priestley's "ugly experiment," is stated by him in a letter to Black, on the ground that

this experiment rendered "the theory useless, in so far as relates to the change of water into air. . . . I have not given up my theory [that is as to the mutual convertibility of water and air], though neither it, nor any other known one will account for this experiment."

In the meantime, Cavendish had been pursuing his inquiries, and towards the end of this year (1783) he was prepared to give the explanation of the cause of the disturbing factor in his proof of the real nature of water, that is, the origin of the occasional and apparently haphazard presence of small quantities of nitric acid. This he demonstrated to be due to the difficulty of excluding a greater or less quantity of atmospheric nitrogen from the gases employed, and he determined the conditions under which this nitrogen led to the formation of the acid, the true nature of which he thus for the first time established. The account of his labours was read to the Royal Society on 15th January 1784.

In the previous autumn, however, disquieting rumours reached this country that the French philosophers, and chief among them Lavoisier, were poaching upon the English preserves. This circumstance is alluded to in a letter from Watt to Deluc, dated November 30, 1783. "I was at Dr. Priestley's last night. He thinks, as I do, that Mr. Lavoisier, having heard some imperfect account of the paper I wrote in the spring, has run away with the idea and made up a memoir hastily, without any satisfactory proofs. . . . I, therefore, put the query to you of the propriety of sending my letter to pass through their hands to be printed; for even if this theory is Mr. Lavoisier's own, I am vain enough to think that he may get some hints

from my letter, which may enable him to make experiments and to improve his theory, and produce a memoir to the Academy before my letter can be printed, which may be so much superior as to eclipse my poor performance, and sink it into utter oblivion; nay, worse, I may be condemned as a plagiary, for I certainly cannot be heard in opposition to an Academician and a Financier. . . . But after all, I may be doing Mr. Lavoisier injustice."

That Lavoisier did get some hints, and possibly even through the medium of Watt's letter, is beyond all question. The fact that he was informed of Cavendish's work is specifically stated in Cavendish's memoir, in a passage interpolated by Blagden, the secretary of the Royal Society, and Cavendish's assistant and amanuensis, who himself had told Lavoisier. The whole of the circumstances are set out in detail in a subsequent letter which Blagden addressed to the editor of the *Chemische Annalen* in 1786. That it was known to be Cavendish's experiment that was being thus repeated is confirmed by a letter from Laplace to Deluc, dated June 28, 1783, in which we read, "Nous avons répété, ces jours derniers, Mr. Lavoisier et moi, devant Mr. Blagden, et plusieurs autres personnes, l'expérience de Mr. Cavendish sur la conversion en eau des airs dephlogistiqué et inflammable, par leur combustion. . . . Nous avons obtenu de cette manière plus de 2½ gros d'eau pure, ou au moins qui n'avoit aucun caractère d'acidité, et qui étoit insipide au goût; mais nous ne savons pas encore, si cette quantité d'eau représente le poids des airs consumés; c'est une expérience à recommencer avec toute l'attention possible, et qui me paroit de la plus grande importance." The phrase "qui n'avoit aucun caractère

d'acidité" is of special significance. The French philosophers, and Lavoisier in particular, could with difficulty, as Blagden relates, be brought to credit the statement that when inflammable air was burnt water only was formed: their preconceptions concerning the part played by oxygen in such a case led them to suppose that an acid would be produced. Cavendish was familiar with Lavoisier's doctrine, which is connoted in the very word oxygen, which we owe to the French chemists, and it may be that this circumstance, amongst others, was one cause of the pains he took to understand the origin of the acid he occasionally met with. Lavoisier was led to repeat Cavendish's experiment on 24th June 1783, and on the following day he announced to the Academy that by "the combustion of inflammable air with oxygen very pure water" was formed. It is this statement that has been said to constitute Lavoisier's claim to be considered as the true and first discoverer of the composition of water. That he has no valid claim has been implicitly admitted by Lavoisier himself. The eminent Perpetual Secretary of the French Academy, M. Berthelot, is no doubt accurate in regarding the 25th June 1783 as the first certain date of publication of the discovery that can be established by authentic, *i.e.* official, documents, but as I have elsewhere attempted to show, the circumstances under which that priority of publication was secured give Lavoisier no moral right to the title of the discoverer.[1]

Shortly after the reading of Cavendish's memoir to the Royal Society (January 15, 1784), Deluc wrote to Watt, giving an account of its contents, and insinuating

[1] Priestley, Cavendish, Lavoisier, and *La Révolution Chimique:* the Presidential Address to the Chemical Section of the British Association, 1890: see also p. 151 of the present volume.

that its conclusions had been formed in the light of knowledge obtained from Watt's letter to the Royal Society, which although, as we have seen, not publicly read, had, there is no doubt, been perused by others than Priestley, to whom it was originally addressed. Deluc was, no doubt, a zealous friend, but in this matter his zeal outran his discretion. The letter was indeed unworthy of him. He hastens to exculpate Lavoisier and Laplace, but he makes a charge against the honour and integrity of Cavendish for which there was absolutely no justification. He stirs up Watt's suspicions, and then seeks to appease them; he rouses his anger, and then counsels him to silence by an argument which shows how wholly he misunderstood Watt. Watt's reply was characteristic: "On the slight glance I have been able to give your extract of the paper, I think his theory very different from mine; which of the two is the right I cannot say: his is more likely to be so, as he has made many more experiments, and consequently has more facts to argue upon.

"As to what you say of making myself *des jaloux*, that idea would weigh little; for were I convinced I had had foul play, if I did not assert my right, it would either be from a contempt of the modicum of reputation which could result from such a theory; from the conviction in my own mind that I was their superior; or from an indolence that makes it easier to me to bear wrongs than to seek redress. In point of interest, in so far as connected with money, that would be no bar; for though I am dependent on the favour of the public, I am not on Mr. C. and his friends; and could despise the united power of *the illustrious house of Cavendish*, as Mr. Fox calls them.

"You may, perhaps, be surprised to find so much

pride in my character. It does not seem very compatible with the diffidence that attends my conduct in general. I am diffident, because I am seldom certain that I am in the right and because I pay respect to the opinion of others, where I think they may merit it. At present *je me sens un peu blessé;* it seems hard that in the first attempt I have made to lay anything before the public, I should be thus anticipated."

There was no desire on the part of anybody connected with the management of the Royal Society to withhold from Watt his just due, and it was eventually arranged that his letter to Priestley, together with one he subsequently addressed to Deluc, should be publicly read to the Fellows, and they were subsequently ordered to be printed in the *Philosophical Transactions* in such manner as their author might desire. By his directions, the two letters were merged together, and they appear as having been read on April 29, 1784, under the title, "*Thoughts on the Constituent Parts of Water, and of Dephlogisticated Air: with an account of some experiments on that Subject. In a letter from Mr. James Watt, Engineer, to Mr. Deluc, F.R.S.*" The greater part of the "thoughts" are concerned with the dephlogisticated air; what relate to water have already been given in the extracts from his correspondence. The terms of the letter to Deluc, as printed in the *Philosophical Transactions*, are substantially identical with those of the letters to Black, Hamilton, Smeaton, and Fry.

I have now given all the essential facts which led to the recognition of the true chemical nature of water, and I have stated as accurately and as impartially as I could the relative shares of Watt, Cavendish, and Lavoisier in their discovery and interpretation. As regards Lavoisier, it cannot be claimed that he was the first to obtain the

facts. To Cavendish belongs the merit of having first supplied the true experimental basis upon which accurate knowledge could alone be founded. Watt, on the other hand, although reasoning from imperfect and indeed altogether erroneous data, was the first, so far as we can prove from documentary evidence, to state distinctly that water is not an element, but is composed, weight for weight, of two other substances, one of which he regarded as phlogiston and the other as dephlogisticated air. It would be a mistake, however, to suppose that Watt taught precisely the same doctrine of the true nature of water that we hold to-day. Nor did Cavendish utter a more certain sound. What we regard to-day as the expression of the truth, we owe to Lavoisier, who stated it with a directness and a precision that ultimately swept all doubt and hesitation aside—except to the mind of Priestley, whose "random experiment" gave the first glimmer of the truth.

In this respect the conclusion of Lord Brougham is most just. It was a reluctance to give up the doctrine of phlogiston, a kind of timidity on the score of that long-established and deeply-rooted opinion, that prevented both Watt and Cavendish from doing full justice to their own theory; while Lavoisier, who had entirely shaken off these trammels, first presented the new doctrine in its entire perfection and consistency.

We thus see that each of these eminent men took an independent and, we may say, an equally important share in the establishment of one of the greatest scientific truths that the eighteenth century brought to light.

As regards Watt, the history of this incident serves to bring out only more clearly what we know to be the true character of the man. It illustrates the vigour of his intellectual grasp, the keenness of his mental vision.

At the same time, it exhibits his love of truth for truth's sake; his unaffected modesty, and the sense of humility that was not the less real because accompanied by a sense of what his inherent love of rectitude taught was due also to himself. The voice of envy and detraction has not been unheard amongst the strife of partisans in the Water Controversy, but throughout it no syllable has been breathed that reflected even remotely upon his honour and integrity.

VI

ANTOINE-LAURENT LAVOISIER

Contemporary Review, December 1890

"Il a été assez heureux ou assez sage, pour que l'on ne sache presqu'autre chose de lui, et qu'il n'y ait dans son histoire d'autre incidens que des découvertes." These words were spoken by Cuvier, the Perpetual Secretary of the French Academy, on the occasion of his *éloge* on Cavendish, the discoverer of the compound nature of water, who, in his old age, had been elected a member of the Institute. At first sight they may seem a mere paraphrase of a saying which has become almost trite, but to those who heard them for the first time they had a significance which must have been realised with something like a pang. For at such a time, not one of Cuvier's hearers could have been unmindful of 1794, or have been unmoved by the recollection of a tragedy in which the most illustrious of Cavendish's contemporaries, a man whose life had been dedicated to the cause of humanity, and whose services to science have reflected an imperishable lustre upon France, was sacrificed to the blind fury of his countrymen. Indeed, to the lively and sympathetic intelligence of such an auditory, quickened as it must have been by the singular charm of the speaker's style, his profound sensibility, and rhetorical skill, the strong dramatic element in the

situation could hardly have remained unperceived. Lavoisier and Cavendish were, in a sense, national types; they were, too, when at the summit of their intellectual power, the acknowledged representatives of two opposing schools of thought. Both were aristocrats, and both, from being poor, became very rich: Cavendish, indeed, was, as M. Biot has said, "le plus riche de tous les savans et probablement aussi le plus savant de tous les riches." But here the resemblance ends: in character, temperament, and genius, in everything that constitutes individuality, the men were as wide asunder as the poles. Cavendish has been described by his biographer Wilson as the most passively selfish of mortals—a sort of scientific anchorite, who maintained, during the fourscore years of his existence, a rigid, undeviating indifference to the affairs of his fellow-men. This embodiment of a clear, cold, passionless intelligence was dead to every æsthetic sense, and had no element of anything that was enthusiastic or chivalrous in its composition. To Cavendish science was, in truth, measurement. "His Theory of the Universe," says Wilson, "seems to have been that it consisted *solely* of a multitude of objects which could be weighed, numbered, and measured; and the vocation to which he considered himself called was to weigh, number, and measure as many of these objects as his allotted threescore years and ten would permit. He weighed the Earth; he analysed the Air; he discovered the compound nature of Water; he noted with numerical precision the obscure actions of the ancient element, Fire." But all this work was done primarily for himself, and to satisfy the questionings of his own intelligence. To give the results of it to the world was hardly a part of his plan, for he cared nothing for the world, and was absolutely indifferent to the interests or

judgment of his fellows. And yet Cavendish was revered, even if he was not loved, during his long and uneventful life, and at his death was laid to rest with every mark of honour and respect in the splendid tomb which his ancestress, Elizabeth Hardwicke, had built for herself and her descendants.

On the other hand, Lavoisier was a man in whom the elements were kindly mixed. No one could more truly say of himself, "Homo sum: humani nihil a me alienum puto." He was ardent, enthusiastic, fond of the society of his fellows, a man of generous impulses, of catholic tastes, and of lofty aims. As a philosopher his influence throughout Europe was supreme, and the manner in which his renown was won was of a kind to strike the national imagination and to minister to the national pride. At the zenith of his fame he was as much a Dictator in the world of science as Napoleon became in the world of politics. But in the very plenitude of this power he was struck at by Fouquier-Tinville, and he who had laboured unceasingly for the glory and well-being of his country was declared guilty of complicity in a conspiracy "against the French people tending to favour by all possible means the success of the enemies of France." Lavoisier's crime was that he had been a *Fermier-général*. He was accused, in the words of the indictment, "of adding to tobacco water and other ingredients detrimental to the health of the citizens." It was a feeble enough weapon to throw even at a *Fermier-général*, but it was thrown with terrible effect. Even to be suspected of tampering with the tobacco of a "citizen" sufficed for the tribunal before which he was brought, although it taxed the ingenuity of Liendon to show how this alleged sophistication brought the accused within the same section of the penal code that swept the

Dantonists to the scaffold. Coffinhal, the Vice-President of the Tribunal, pronounced the judgment: "The Republic has no need of men of science," and within twenty-four hours the tumbrel was on its way to the Place de la Révolution, and, as the *procès-verbal* sets forth, "sur un échafaud dressé sur la dite place, le dit Lavoisier, en notre présence, subi la peine de mort." Well might Lagrange say to Delambre: "It required but a moment to strike off this head, and probably a hundred years will not suffice to reproduce such another."[1]

The main events in the scientific career of the great French chemist are tolerably well known, and his position in the history of the development of chemistry is now fully assured. The story of his scientific life has recently been told by M. Berthelot with all the charm and tact which characterise the *éloges* which it is the duty of the secretaries of the Academy from time to time to prepare. Although English men of science may think that M. Berthelot occasionally fails to mete out the strict justice to their countrymen that historical accuracy demands, there cannot be a doubt, in spite of all legitimate deductions, that Lavoisier remains the dominant figure in the chemical world of the eighteenth century. There is much, however, in his life and work, and especially in the circumstances which led to his untimely death, on which we would gladly have more information. Amongst the crop of literature which the centenary of the Revolution has brought forth in France, the historian of science has welcomed, therefore, with special interest,

[1] The Republic, a few months afterwards, found also that it had no need of Coffinhal: he fell with Robespierre, and was guillotined on the 18th Thermidor of the year II. Fouquier-Tinville and some half-dozen others who had been concerned in the trial of Lavoisier were also brought to the scaffold at about the same time.

the admirable monograph on Lavoisier which we owe to the patient industry and patriotic zeal of Professor Grimaux.[1]

It must have struck many people, as M. Grimaux tells us it has struck him, that, in spite of the glory which surrounds the name of Lavoisier, it is remarkable that the life of the creator of modern chemistry had still to be written. Beyond the short biographical notices by Lalande, Fourcroy, and Cuvier, which deal mainly with Lavoisier as a man of science, we know practically nothing of the story of a life which was wholly devoted to the public good. Even the world of science knows scarcely anything of his private life, of his virtues, of his intelligent philanthropy, and of the services which he rendered to his country as an academician, an economist, an agriculturist, and a financier. Luckily for his biographer, Lavoisier was a man of perfect method, and he preserved all his manuscripts without exception. After his death these papers were religiously guarded by Madame Lavoisier, from whom they passed to Madame Léon de Chazelles, her grandniece. This, together with other material preserved at the Château de la Canière, where also are kept Lavoisier's books and instruments, has served M. Grimaux as the basis of his book. In addition, he has searched through the public archives, with the result that we have now presented to us for the first time the details of Lavoisier's political life and the true story of his trial and condemnation.

Antoine-Laurent Lavoisier was born in Paris on 26th August 1743. His father, Jean-Antoine, was an advocate; his mother, *née* Punctis, died when he

[1] *Lavoisier*, 1743-1794. "D'après sa correspondance, ses manuscrits, ses papiers de famille, et d'autres documents inédits." Par Edouard Grimaux. Paris: Félix Alcan, 1888.

was five years old, and he and a young sister, who lived only a few years, were taken charge of by the grandmother and her daughter, Mdlle. Constance Punctis. The family was rich, and was able to send the young Antoine to the Collège Mazarin, where he seems to have acquired that passion for natural science which was the motive power of his life. He studied mathematics with the Abbé La Caille, well known for his measurement of an arc of the meridian at the Cape of Good Hope, and for his determination of the length of a seconds pendulum; he learnt botany from Bernard de Jussieu, and geology and mineralogy from Guettard. But it was principally by Rouelle's teaching that the particular direction of Lavoisier's scientific activity was shaped. Guillaume-François Rouelle is mainly remembered by chemists to-day as having just missed the discovery of the Law of Combination by Definite Proportions. By his contemporaries he was considered to have said more "good things" than any man of his time. In spite of his oddities, he exercised an extraordinary influence as a teacher; his lecture-room at the Jardin du Roi was crowded by auditors from all parts of Europe, and among his pupils were Macquer, Bucquet, d'Arcet, and Lavoisier, the men who were destined to make the end of the eighteenth century one of the most stirring epochs in the history of chemistry.

Lavoisier was originally intended for the profession of the law, and actually became a licentiate in 1764, but at the instigation of Guettard, whom he accompanied in his journeys through France, and to whom he was of assistance in the preparation of his great Mineralogical Atlas, he abandoned that career and gave himself up to science. In 1765 he sent his first

paper to the Academy—a modest enough communication on gypsum, but noteworthy as giving for the first time the true explanation of the setting of plaster of Paris, and of the reason that overburnt gypsum will not rehydrate.

In the following year he was awarded a medal by the Academy for an essay on the lighting of large towns, and in the same year he was placed upon the list of candidates for election into that august body. The Académie des Sciences has suffered frequent internal changes, but in the middle of the eighteenth century it was subject to the constitution of 1699, as modified in 1716. It was composed of members of very different orders, enjoying very unequal rights. There were twelve honorary members chosen from among the great nobles, and from whom were selected the president and vice-presidents; eighteen pensionaries, twelve associates, and twelve *adjoints* distributed among the geometers, astronomers, mechanicians, chemists, and botanists; in addition there were a number of free associates, superannuated associates, and pensionaries. The honorary members and the pensionaries had alone a deliberative voice in the elections, and in the business of the Academy. The two associates in the class in which there was a vacancy were, however, called upon, in company with three pensionaries, to draw up the list of candidates. The *adjoints* had practically no privileges beyond that of sitting next to an associate when there was room; at other times they sat upon the benches placed behind the arm-chairs of the associates.

The 18th of May 1768, when the young Lavoisier gained his seat upon the back benches, was a red-letter day in the history of the house of Punctis. It was

no less important in the history of the Academy, for the young *adjoint* was destined to be its champion and do battle for its existence during the dark and terrible time of the Revolution. Lavoisier's extraordinary power of work, his intellectual keenness, and range of knowledge, were quickly recognised, and in spite of his youth he was charged with the preparation of numerous Reports. This part of an academician's duty was as difficult and irksome as it was delicate. During the twenty-five years of his connection with the Academy he contributed upwards of two hundred reports on such disconnected matters as the theory of colours, the areometer of Cartier, modes of determining longitudes, arm-chairs for invalids, prison reform, water supply, the cold of the winter of 1776, the pretensions of Mesmer, the aerostatic inventions of Montgolfier, the imposture of the divining-rod, etc., etc.

Almost immediately after Lavoisier had thus planted his foot on the ladder of fame, he set it unconsciously on the first step of the scaffold. *Adjoint* of the Académie des Sciences, he now became *adjoint* of the *Ferme-général*. His friends, the academicians, looked somewhat askance at this action. Lalande tells us that in his election they had been influenced by the consideration that a young man of parts and activity, whose private means placed him beyond the necessity of seeking another profession, would naturally be useful to science, and they now feared that the new duties would clash with what they imagined was to be the real work of his life. But, luckily, there are always some to offer consolation. "Tant mieux!" said the geometer Fontaine, "the dinners that he will give us will be all the better." Although Lavoisier had inherited his mother's fortune, it was hardly sufficient for the career

which he now planned for himself, and by the advice of a friend of the family, M. de La Galaizière, he became *adjoint* of the *Fermier-général* Baudon, in return for a third of his interest in the lease of Alaterre.

But there were doubtless other reasons for the disapproval of the Academy. The *Ferme-général* was a part of the rotten financial system which ultimately landed France in disaster. It was a company of financiers, to whom the State conceded, for a fixed annual sum, payable in advance, the right of collecting the indirect taxes of the country. Created originally by Colbert, its constitution and functions were modified by successive finance-ministers during the reigns of Louis XIV. and Louis XV., as the will of the King, or the exigencies of the national Exchequer determined. At the time that Lavoisier entered it, the number of the *Fermiers-généraux* was sixty, and the sum to be paid in advance for the lease of six years was 90,000,000 livres, together with a *douceur* of 300,000 livres for the Controller-General. The *Fermiers-généraux* received sums on account during the continuance of the lease, but the actual result of the speculation was known only at its expiration. They had to bear all the expenses of management and collection, to enforce the customs and excise regulations, and their profits were subject to all sorts of rebates, perquisites, pensions, and *pots-de-vin*. It need hardly be said that in the time of the Grand Monarch and his worthy great-grandson, the *Ferme* was a very hotbed of jobbery, corruption, and malversation. There existed no public audit; none, indeed, was possible. Even the Finance Minister could gain but little information of the details of its monetary transactions. In 1774, Terray, towards the conclusion of the first

lease in which Lavoisier was interested, addressed a confidential inquiry to the *Fermiers-généraux* as to the number of beneficiaries which the will of the Court, *i.e.* the king or his mistresses, had imposed upon the *Ferme-général.* Through the indiscretion of a clerk the list was made public. Paris was scandalised to learn that the pensions alone amounted to upwards of 400,000 livres annually. In addition, the king secured a sixtieth share of the property of the *Ferme*; his sisters and aunts disposed of 50,000 livres; the nurse of the Duke of Burgundy, 10,000 livres; Madame du Barry's physician, 10,000 livres; the Abbé Voisenon, 3000 livres; a court singer, 2000 livres; and so on. Altogether, the Court and its creatures netted in this way fourteen-sixtieths of the proceeds of the lease of Alaterre. Many of the *Fermiers-généraux* themselves outraged public opinion by their prodigality and the luxury of their hotels and *petites-maisons.* The organisation was detested throughout the length and breadth of France. The peasants, too far from the capital to hear the curses which Mercier flung at the Hôtel des Fermes, were constantly witness of the hardships it inflicted, and the terrible retribution which followed any contravention of its decrees. The taxes were most unequally levied; each province had its own frontier, and to a population impoverished and on the verge of starvation there was every temptation to smuggle; conflicts with its officers were of almost daily occurrence; no house was safe against domiciliary visits, and hundreds of persons were yearly sent to the galleys for the most trifling acts of contraband. It is true there was the *Court des aides,* to which the peasant might appeal against the imposition of the *Ferme,* but too frequently he found that the "gratuitous justice" thus dealt out to him meant only

"justice by gratuities." Nor was it only on the frontiers that smuggling prevailed. It was calculated that at least one-fifth of the merchandise that entered Paris was contraband. To render the collection of the *octroi* more certain, and to check irregularities, the *Ferme* proposed to surround the city with a wall. Public feeling against the project was intense. A wit of the period declared that "le mur murant Paris rend Paris murmurant." Military opinion also was adverse to the proposal; the Duke de Nivernais, a Marshal of France, is reported to have said that its author deserved hanging from one of his own towers; and Marat subsequently denounced Lavoisier as the originator of what the citizens were taught to regard as an ingenious method of robbing them of the pure air of the country.

There were, of course, honest *Fermiers-généraux*—men like Delahante, Paulze, d'Arlincourt, and others, and Lavoisier was of the number, who discharged their trust honourably, and who sought to introduce order and good management into the affairs of the society. With the advent of the better times which the beginning of the reign of Louis XVI. seemed to promise, and under the administration of Turgot, the character of the *Ferme-général* improved. With each new lease the position and influence of Lavoisier was strengthened, until, in 1783, he was placed by d'Ormesson upon the Committee of Administration, the most important of the whole, and the only one which had direct relations with the Government. He was thus enabled to remedy many abuses, and to mitigate in various ways the burden of imposition under which France groaned. But it was too late. Nothing the *Ferme* could do would ever wipe out the memories of its exactions. With the growth of Lavoisier's power and influence in the *Ferme*,

the odium with which it was regarded seemed gradually to concentrate itself upon him. His rectitude, his public services, the purity of his private life, the splendour of his scientific achievements, were unheeded. In the day of reckoning he was remembered only as Lavoisier the *Fermier-général.*

M. Grimaux has been at considerable pains to lay the details of Lavoisier's connection with the *Ferme-général* before us. He estimates that, in all, he acquired, from 1768 to 1786, nearly 1,200,000 livres. He continued to be a member of the *Ferme* until it was suppressed by a decree of the National Assembly in 1791, when its liquidation was confided to six of his colleagues.

Lavoisier's success in administration induced Turgot to consult him on the means of ensuring a regular supply of gunpowder for the service of the State. Prior to Turgot's ministry the manufacture of the gunpowder required for the national defence was entrusted to a financial company, with the result that, on more than one occasion, France was obliged to sue for peace from inability to provide herself with the munitions of war. The *Ferme des Poudres* was managed solely in the interest of its members: waste, peculation, and jobbery were as rampant as in the old days of the *Ferme-général.* Turgot changed all this. In 1775 he created the *Régie des Poudres* and nominated four commissioners, who should be directly responsible to the State for the manufacture of gunpowder. Lavoisier is expressly named as one of the commissioners, as being known, not only for his chemical knowledge, so necessary for administrative work of this kind, but also for the diligence, capacity, and honesty with which he discharged his duties as a *Fermier-général.* At his sugges-

tion, Turgot invited the Academy to offer a prize for the best essay on the economical production of saltpetre, with a view of obtaining information on the modes of manufacture practised in various parts of Europe. No detail of administration was too minute to escape his attention. He regulated the conditions under which the *employés* were selected; he simplified the methods of manufacture and refining of saltpetre, and by successive improvements in composition and modes of mixing he greatly increased the ballistic properties of gunpowder. He who was condemned in 1794 as an enemy to his country was in 1780 recognised as having contributed to its triumphs by augmenting the force of its arms. At times the exercise of his duties placed him in considerable danger, as, for example, in the explosion which resulted in the experiments made to introduce Berthollet's newly discovered chlorate of potash in the place of nitre. But no gunpowder-mill under Lavoisier's charge was half so explosive as Paris in 1789. The events of July had demoralised the city, and it was only too ready to give heed to the slanders and coarse invective of the *Père Duchesne* of Marat and of other self-styled "Friends of the People." The air was full of plots and counter-plots. An order to transport some gunpowder was maliciously misconstrued; the report was spread that it was to be given to the enemies of the nation, and Lavoisier and his fellow-commissioner, Le Faucheux, nearly fell victims to an angry mob which surged round the gates of the arsenal.

Lavoisier's journeyings through France in connection with the work of the Mineralogical Atlas and as a *Fermier-général*, had taught him much concerning the life of the peasant. Indeed, no Frenchman of his time knew his country better, or was more keenly alive to

the vast economic movement which was slowly gathering strength during the latter half of the eighteenth century. His interest in this movement was no doubt quickened by, even if it did not originate in, his connection with the *Ferme*. It was obvious to him that the whole fiscal system of the country fell with the most crushing effect upon the class least able to bear it, and in the numerous commissions in which he took part he repeatedly indicated the economic disadvantages with which the cultivators of the soil had to contend. In 1785 he became a member, and immediately afterwards secretary of the Committee of Agriculture—a consultative body created by Calonne for the purpose of enlightening the controller-general on agronomic matters in general. Lavoisier not only held the pen; he was the directing spirit of the Committee. He drew up reports and instructions on the cultivation of flax, of the potato, on the liming of wheat; he prepared a scheme for the establishment of experimental farms, and for the collection and distribution of agricultural instruments, for the better adjustment of the tithes and of the rights of pasturage, etc. He was no mere theorist in these matters. In 1778, when he acquired the demesne of Fréchines, the condition of the peasant was wretched in the extreme. Cultivated grazing land was unknown; the farmers, from the impossibility of feeding their cattle during the winter, had but few beasts; the fields were unmanured; and the yield of corn, even in the best years, was only about five times the weight of the seed. With a view of demonstrating how much might be done by improved methods of tillage, he decided to make trials on above 80 hectares of the worst land on the demesne; and he divided about 240 hectares into three farms, of which he directed the cultivation with all the

precision of laboratory trials. He introduced the cultivation of the beetroot and potato, hitherto unknown in the Blésois. He improved the breed of sheep by the importation of rams and ewes from Spain, and that of cows by the introduction of animals from the model farm of Chanteloup. In 1788, when he presented to the Society of Agriculture the results of his ten years' experience, he again set forth the various disadvantages under which the cultivator laboured — short leases, insecurity of tenure, want of capital, and of credit; and he made a strong appeal to the proprietors to spend more on the amelioration of their land in order to improve the condition of those who were obliged to live upon it. Each succeeding year saw a change for the better in the lot of the peasants at Fréchines. In 1793 the crop of wheat had doubled itself, and was more than ten times the weight of the seed, and the number of beasts on the estate had increased fivefold. In the following year came the end, but the memory of the man who was a veritable father of his people is still cherished in the district of Blois.

Lavoisier's position as a landed proprietor was doubtless the cause of his selection as a member of the Assembly of the Orléanais, a sort of County Council created in 1787, according to a plan devised by Necker during his first administration. It was composed of twenty-five members selected by the king, six for the clergy, six for the nobility, and twelve for the third estate, together with the Duke of Luxembourg as president. The twenty-five so nominated were directed to elect an equal number of colleagues, the same proportion being observed for the three orders. The duties of the Assembly were to determine the modes of levying the taxes, to undertake the construction and maintenance

of the highways, and to consider how the commerce and industry of the province might best be developed. Lavoisier, although an esquire, was chosen as a member of the third estate, and he at once became the leader of that section. In the town library of Orleans are preserved the minutes of the Provincial Assembly, together with such of the manuscripts of Lavoisier as relate to its business. During the greater part of its existence the Assembly was engaged in attempts to settle the mode of incidence and collection of the taxes. The third estate demanded the abolition of the exemptions enjoyed by the nobles; the substitution of a fixed subscription for the tithes, which fell with especial severity on the smaller proprietors; and the abolition of the *corvée*, which compelled the peasants to undertake the construction and maintenance of the roads. On all these questions Lavoisier was the mouthpiece of his order. He also introduced schemes for the founding of saving and discount banks, workhouses, and insurance societies, for the creation of nursing establishments, for the formation of canals, and for the exploitation of the mineral productions of the province. "Celui qui fait tout, qui anime tout, qui se multiplie en quelque sorte, c'est Lavoisier; son nom reparaît à chaque instant."[1]

Few, if any, of these projects were realised. The Provincial Assemblies might initiate, but they were powerless to execute, and in 1790 they became merged into the States-General, to which Lavoisier was sent as *Député suppléant* for the bailiwick of Blois, having for his colleague the unfortunate Vicomte de Beauharnais, whose widow, Josephine Tascher de la Pagerie, became the wife of Bonaparte. In the same year he was elected a member of the Commune of Paris, and of the

[1] Leonce de Lavergne, *Les Assemblées Provinciales.*

famous club of 89, of which he was ultimately secretary. This institution, which sought to develop, defend, and propagate the principles of constitutional liberty, numbered amongst its adherents all who were eminent in art, literature, science, and politics in France. It had, however, but little influence on the main currents of political thought, and absolutely none on the political action of the time; it dealt too largely with questions of political metaphysics to stem the forces which ultimately gained an overwhelming strength. It ended by being suspected of "aristocratism," and it became a crime to have been one of its members. At the beginning of 1794 the Jacobins expelled from their club all who had been part of the Society of 89 as, *ipso facto*, guilty of "incivism."

Dark clouds were now rapidly gathering; the days of the Great Terror were approaching, and Lavoisier found himself menaced on every side. The first attack came from Marat. Marat had sought, at the outset of his career, to make a name in science by publishing a Treatise on Fire, full of the crudest and most ridiculous speculations on the nature of combustion, and which he caused the *Journal de Paris* to announce had been received with approbation by the Academy. The statement was absolutely baseless, and Lavoisier, as director of the Academy, said so in a few disdainful words. Marat now vented his rage on the Academy, and in a miserable pamphlet traduced men like Laplace, Monge, and Cassini, accusing them of malversation, and of spending in disgraceful orgies the sums voted for the study of aerostation. But it was specially on Lavoisier that he concentrated all his venom and rancour. "Lavoisier, the putative father of all the discoveries which are noised abroad, having no ideas of his own,

fastens on those of others ; but, incapable of appreciating them, he abandons them as readily as he adopts them, and changes his systems as he does his shoes ! "

In his paper, the *Ami du Peuple*, he is even more furious :—

> I denounce this Corypheus of the Charlatans, Sieur Lavoisier, son of a land-grabber (*grippe-sol*), chemical apprentice, pupil of the Genevese stock-jobber, fermier-général, régisseur of powder and saltpetre, administrator of the Discount Bank, secretary of the king, member of the Academy of Sciences. . . . Would to heaven that he had been strung to the lamp-post on the 6th of August. The electors of La Culture would then not have to blush for having nominated him.

At the same time, Lavoisier, as *Fermier-général*, was the object of repeated and violent attacks in the journals and in various political clubs. The leaders of the Revolutionary party, who clamoured for the abolition of all State control over the manufacture of war material, denounced his administration at the *Régie des Poudres*, and he was shortly afterward removed by the National Assembly. The king, however, so far intervened in his behalf as to order that he should be allowed to remain in undisturbed possession of his rooms in the Arsenal, where he had established a laboratory, on which he had expended a large portion of his private fortune. He had been appointed a member of the National Treasury in 1791, but in 1793, to the regret of his colleagues, he requested to be relieved of his functions. In truth, the strain of a constant anxiety was beginning to react upon him ; he was becoming weary of the incessant struggle against enemies who were as vindictive as they were unscrupulous, and longed for the peace and quietude which he found only in his laboratory. To have property was, in the eyes of the Revolutionary tribunals, to be

guilty of "incivism"; and "incivism" was a crime against the Republic. Lavoisier told Lalande that he expected to be stripped of everything, but he added he was not too old to work, and he would begin life again as an apothecary.

On quitting the Treasury, he was reappointed to the *Régie des Poudres*, but a few months afterwards he resigned the position, although he engaged to continue his studies on the manufacture of powder, and on the methods for the analysis of nitre. It is possible that he may have had some warning of what followed. Three days after his resignation, a commission of the Assembly suddenly entered the Arsenal, placed the papers under seal, and ordered the removal of the commissioners to La Force. The elder Le Faucheux, one of Lavoisier's colleagues, enfeebled by age and infirmities, killed himself in despair, and the son was thrown into prison.

But however desirous Lavoisier might have been to relinquish political life, it was impossible for him to escape from the penalties and responsibilities of his position. In 1791 he had been named secretary and treasurer of the famous Commission of Weights and Measures, which had undertaken to realise the long-cherished idea of supplying France and the world with an international system of weights and measures based upon a natural unit. He was not only the administrative officer of the Commission; he contributed to the nomenclature of the system, and took a prominent part in directing the determination of the various physical constants on which the measurements ultimately rested, and especially in the determination of the weight of the unit volume of water, on which the value of the standard of mass was based. The project

had to be carried out under conditions which could not possibly have been more disadvantageous. Its realisation largely depended on the cordial co-operation of other nations, and the work of measurement could only properly be conducted at a time of peace. France was torn and distracted by internal dissensions; her national credit was gone; and she was threatened on all sides. Delambre has left us an account of the extraordinary difficulties and dangers under which the geodetical observations were executed. Lavoisier's work in Paris as treasurer was hardly less onerous or less hazardous. The project was more than once imperilled by the vacillating action of the Convention. The sums voted by the Assembly were not always forthcoming from the Treasury, and Lavoisier was occasionally under the necessity of depending upon his own means, or his private credit, for the money which Méchain required in order to extend the measurement of the arc to Barcelona.

Doubtless, much of the difficulty was due to the attitude of the Convention towards the Academy. In turn with every monarchical institution of the time, the Academy was suspected of "incivism," and its destruction was already being compassed. Lavoisier, who had been named treasurer in succession to Tillet, whose long illness had thrown the financial affairs of the learned body into confusion, now found himself confronted with new troubles. The salaries of the Academicians, many of whom were old men, and in straitened circumstances, were in arrears. Lavoisier was again under the necessity of advancing money from his private purse in certain of the more urgent cases. The Society continued to hold its meetings as usual until the spring of 1792, when an unexpected

motion by Fourcroy revealed to the Academicians the danger in which they stood. Fourcroy demanded that the Academy should expel such of its members as were known for their "incivism." The motion was resisted on the ground that the Academy had no concern with the political opinions of its members: the progress of science was its sole business. Fourcroy insisted on his motion, when the geometer Cousin found the way of escape from a position which it was evident had been most skilfully chosen, by proposing that the question should be submitted to the Minister, who would make such erasures from the list as he thought necessary, whilst the Academy should continue to pursue its more intellectual occupations.

This suggestion was adopted, but Fourcroy was not a man to submit tamely to a rebuff, and the Academy soon felt the effect of his resentment. Although the responsible Ministers of the Government still recognised it as the intellectual centre of France, its enemies within the Convention were unceasing in their efforts to overthrow it. The outlook was gloomy in the extreme. The shadow of its impending doom seemed to hang over its meetings. We find at this time in its minutes no mention of the members present, nor of the discussions in which they engaged. Even during the dark days of 1793, Lavoisier, active, hopeful, and courageous as ever, strained every nerve to maintain the continuity of its work; he was the life and soul of the Society, and the ever-watchful guardian of its interests. Together with Haüy and Borda he laboured incessantly at the work of the Commission. He obtained for Vicq d'Azir 8400 livres for the continuation of his treatise on human and comparative anatomy; Jeurat received 300 livres for the calculations for his new lunar tables;

Berthollet the 100 louis which he required for his work on applied chemistry. Even Sage, one of the most bitter opponents of the new chemistry, and Fourcroy still obtained the money which they needed for the prosecution of their investigations. He exerted all his influence with Ministers, with the administrators of the Directory, and with the commissioners of the Treasury, to induce the Government to fulfil its obligations towards the Academy. The eloquence of Grégoire, and the courage of Lakanal for a time averted the final blow, but the enemies of the Academy eventually found they had a majority in the Convention, and they hastened to make use of it. The painter David pronounced the doom of all the learned societies of France, and on August 8, 1793, the Convention decreed their suppression.

Fourcroy had triumphed; too timorous to work in the open, he had been the unseen power behind the Convention which had steadily undermined the influence of the Academy, and had at length effected its destruction. Still Lavoisier did not despair. He appealed to the Committee of Public Instruction to allow the members of the Academy to continue their labours, and he entreated that the work of the Commission of Weights and Measures might not be interrupted. True to his trust, he pleaded for those of his colleagues who had been reduced to poverty by the decree of the Convention :—

> It is unnecessary to add, citizens, that the continuance of their salaries to those who have earned them is demanded by justice; there is not an academician who, if he had applied his intelligence and means to other objects, would not have been able to secure a livelihood and a position in society. It is on the public faith that they have followed a career, honourable without doubt, but hardly

lucrative. Many of them are octogenarians and infirm; several of them have spent their powers and their health in travel and investigations undertaken gratuitously for the Government; the sense of rectitude of Frenchmen will not allow the nation to disappoint their hopes; they have at least an absolute right to the pensions decreed in favour of all public functionaries. . . . Citizens, the time presses; if you allow the men of science who composed the defunct Academy to retire to the country, to take other positions in society, and to devote themselves to lucrative occupations, the organisation of the sciences will be destroyed, and half a century will not suffice to regenerate the order. For the sake of the national honour, in the interests of society, as you regard the good opinion of foreign nations, I beseech you to make some provision against the destruction of the arts which would be the necessary consequence of the annihilation of the sciences.

The Convention was inexorable and Fourcroy relentless. He now acted as if his object was to crush Lavoisier, and by an adroit move he caused him to be stigmatised as a counter-revolutionist. A few days afterwards the Convention ordered the arrest of Lavoisier, together with the rest of the *Fermiers-généraux* who had signed the leases of David, Salzard, and Mager, and he was conducted to the prison of Port-Libre. Every effort on the part of his friends was put forth to save him. The Commission of Weights and Measures, headed by Borda and Haüy, appealed to the Committee of Public Safety. It refused to discuss the petition, and two days afterwards, on the advice of the Committee of Public Instruction, of which Fourcroy and Guyton Morveau were members, the names of Borda, Lavoisier, Laplace, Coulomb, Brisson, and Delambre were removed from the Commission. The Committee of Assignats requested in vain that Lavoisier might be allowed to work in his laboratory. The Bureau de Consultation des Arts et Métiers, of which Lavoisier was president at the time of his

arrest, addressed a memorial extolling the value of his public services, and drawing special attention to the fact that the scheme of national instruction then before the Convention was entirely of his creation. All was in vain. The Terrorists were in complete ascendency in the Convention. Robespierre had swept Hébert and Danton from his path, and the work of "purification" was going on merrily. On May 8, 1794, the *Fermiers-généraux* were brought to trial, but their condemnation had already been pronounced. Liendon, in the turgid rhetoric of the period, demanded the heads of the prisoners . . . "the measure of the crimes of these vampires is filled to the brim . . . the immorality of these creatures is stamped on public opinion; they are the authors of all the evils that have afflicted France for some time past." Hallé attempted to intervene on behalf of Lavoisier, and presented the memorial of the Bureau de Consultation; Coffinhal, who presided, pushed it aside, with the memorable words: "La République n'a pas besoin de savants; il faut que la justice suive son cours." The twenty-eight *Fermiers-généraux* were found guilty of death. They were sentenced to be executed within twenty-four hours, and it was ordered that their property should be confiscated to the Republic. Such was the haste of the judges that the decision of the jury was omitted from the minute of judgment—an act of informality which Dobsen used with terrible effect a few months later, when Fouquier-Tinville and Coffinhal found themselves in the place of the unfortunate *Fermiers-généraux.*

On the following morning the condemned men were taken from the Conciergerie to the Place de la Révolution. They bade each other farewell; Papillon d'Auteroche, seeing the crowd *en carmagnole* as the

carts passed through the streets, raised a smile as he said disdainfully, in allusion to the confiscation of his effects: "What annoys me is to have such disagreeable heirs." They were guillotined in the order of their names on the indictment. Lavoisier saw fall the head of his father-in-law, and was himself the fourth to suffer. In common with all his companions, he bore himself with dignity, and met his end calmly and with courage. The spectacle of such fortitude awed the crowd into silence, and no reproach or insult reached the ears of the dying man.

Thus perished, at the age of fifty-one, one of the most remarkable men in the history of science. All that was mortal of him was thrown into the cemetery of the Madeleine, and the place of his interment was forgotten. The news of this great crime profoundly affected the intellectual world. There was not a scientific body in Europe that failed to give utterance to its sense of shame and sorrow. With the fall of Robespierre this feeling penetrated France. On October 22, 1795, Lalande pronounced an *éloge* on Lavoisier before the Lyceum of Arts, and in the midst of the extraordinary revulsion of popular feeling which preceded the days of the Directory the same body decreed a solemn funeral ceremony in his honour. It was, in truth, a lugubrious farce, marked by all the extravagances of taste and sentiment which were then in fashion, and it was crowned by an *éloge . . . from Fourcroy!* Time-serving and timorous as ever, Fourcroy had no other extenuation than an appeal to the memories of the Great Terror. "Carry yourselves back to that frightful time . . . when terror separated even friends from each other, when it isolated even the members of a family at their very fireside, when the

least word, the slightest mark of solicitude for the unfortunate beings who were preceding you along the road to death, were crimes and conspiracies." For Fourcroy to plead that he was pusillanimous hardly serves to exculpate him. He would have us believe that he was powerless to avert the catastrophe he now affects to deplore; but he stands charged, on his own showing, with participation in acts which largely contributed to it, and the charge rests heavily on his memory.

VII

PRIESTLEY, CAVENDISH, LAVOISIER, AND *LA RÉVOLUTION CHIMIQUE*

THE PRESIDENTIAL ADDRESS TO THE CHEMICAL SECTION OF THE BRITISH ASSOCIATION, LEEDS, 1890

LEEDS has one most notable association with chemistry of which she is justly proud. In the month of September 1767 Dr. Joseph Priestley took up his abode in this town. The son of a Yorkshire cloth-dresser, he was born in 1733 at Fieldhead, a village about six miles hence. His relatives, who were strict Calvinists, on discovering his fondness for books, sent him to the Academy at Daventry to be trained for the ministry. In spite of his poverty and of certain natural disadvantages of speech and manner, he gradually acquired, more especially by his controversial and theological writings, a considerable influence in Dissenting circles. A pressing invitation and the prospect of one hundred guineas a year induced him to accept an invitation to take charge of the congregation of Mill Hill Chapel here. He was already known to science by his *History of Electricity*, and the effort was made to attach him still more closely to its cause by the offer of an appointment as naturalist to Cook's Second Expedition to the South Seas. But, thanks to the

intervention of some worthy ecclesiastics on the Board of Longitude who had the direction of the business, and who, as Professor Huxley once put it, "possibly feared that a Socinian might undermine that piety which in the days of Commodore Trunnion so strikingly characterised sailors," he was allowed to remain in Leeds, where, as he tells us in his *Memoirs*, he continued six years, "very happy with a liberal, friendly, and harmonious congregation," to whom his services (of which he was not sparing) were very acceptable. "In Leeds," he says, "I had no unreasonable prejudices to contend with, and I had full scope for every kind of exertion." [1]

We have every reason to feel grateful to the "worthy ecclesiastics," since their action indirectly occasioned Priestley to turn his attention to chemistry. The accident of living near a brewery led him to study the properties of "fixed air," or carbonic acid, which is abundantly formed in the process of fermentation, and which at that time was the only gas whose separate and independent existence had been definitely established. From this happy accident sprang that extraordinary succession of discoveries which earned for their author the title of the Father of Pneumatic Chemistry, and which were destined to completely change the aspect of chemical theory and to give it a new and unexpected development.

I have been led to make this allusion to Priestley, not so much on account of his connection with this place as for the reason that, as it seems to me, there has been a disposition to obscure his true relation to the

[1] Leeds still enjoys one of the fruits of Priestley's insatiable power of work in her admirable Proprietary Library. He seems to have suggested its formation, and was its first honorary secretary.

marvellous development of chemical science which made the close of the eighteenth century memorable in the history of learning. Our distinguished fellow-worker, M. Berthelot, the Perpetual Secretary of the French Academy, has recently published, under the title of *La Révolution Chimique*, a remarkable book, written with great skill, and with all the charm of style and perspicacity which invariably characterises his work, in which he claims for Lavoisier a participation in discoveries which we count among the chief scientific glories of this country. From the eminence of M. Berthelot's position in the world of science his book is certain to receive in his own country the attention which it merits, and as it is issued as one of the volumes of the Bibliothèque Scientifique Internationale it will probably obtain through the medium of translations a still wider circulation. I trust that I shall not be accused of being unduly actuated by what Mr. Herbert Spencer terms "the bias of patriotism," in deeming the present a fitting occasion on which to bring these claims to your notice with a view of determining how far they can be substantiated.

All who are in the least degree familiar with the history of chemical science during the last hundred years, will recognise, as I proceed, that the claims which M. Berthelot asserts on behalf of his illustrious predecessor are not put forward for the first time. Explicitly made, in fact, by Lavoisier himself, they were uniformly and consistently disallowed by his contemporaries. M. Berthelot now seeks to support them by additional evidence and to strengthen them with new arguments, and asks us thereby to clear the memory of Lavoisier from certain grave charges which lie heavily on it. You have doubtless anticipated that these claims

have reference to Lavoisier's position in relation to the discovery of oxygen gas and the determination of the non-elementary nature of water.

The substance we now call oxygen—a name we owe to Lavoisier—was discovered by Priestley on August 1, 1774; he obtained it, as every schoolboy knows, by the action of heat upon the red oxide of mercury. We all remember the characteristically ingenuous account which Priestley gives of the origin of his discovery. M. Berthelot sees in it merely the evidence of the essentially empirical character of his work. "Priestley," he says, "the enemy of all theory and of every hypothesis, draws no general conclusion from his beautiful discoveries, which he is pleased, moreover, not without affectation, to attribute to chance. He describes them in the current phraseology of the period with an admixture of peculiar and incoherent ideas, and he remained obstinately attached to the theory of phlogiston up to his death, which occurred in 1804" (p. 40).

Such statements are calculated to give an erroneous idea of Priestley's merit as a philosopher. That the implication contained in the passage is wholly opposed to the real spirit of his work might be readily shown by numerous quotations from his writings. Perhaps this will suffice: "It is always our endeavour, after making experiments, to generalise the conclusions we draw from them, and by this means to form a *theory* or system of principles to which all the *facts* may be reduced, and by means of which we may be able to foretell the result of future experiments." This quotation is taken from the concluding chapter of his *Experiments and Observations on Different Kinds of Air*, in which he actually seeks to draw "general conclusions" concerning the constituent principles of

the various gases which he himself made known to us, and to show the bearing of these conclusions on the doctrine of phlogiston. That he was content to rest in the faith of Stahl's great generalisation, even to the end, is true, and the fact is the more remarkable when we recall the absolute sincerity of the man, his extraordinary receptivity, and, as he says of himself, his proneness "to embrace what is generally called the heterodox side of almost every question." If it is argued that this merely shows Priestley's inability to appreciate theory, it must be at least admitted that there is no proof that he was inimical to it. His position is clearly evident from the concluding words of the section of his work from which I have already quoted: "This doctrine of the composition and decomposition of water has been made the basis of an entirely new system of chemistry, and a new set of terms has been invented and appropriated to it. It must be acknowledged that substances possessed of very different properties may, as I have said, be composed of the same elements in different proportions and different modes of combination. It cannot, therefore, be said to be absolutely *impossible* but that water may be composed of these two elements or any other. But then the supposition should not be admitted without *proof*; and if a former theory will sufficiently account for all the *facts*, there is no occasion to have recourse to a new one, attended with no peculiar advantage (*loc. cit.* p. 543). . . . I should not feel much reluctance to adopt the *new doctrine*, provided any new and stronger evidence be produced for it. But though I have given all the attention that I can to the experiments of M. Lavoisier, etc., I think that they admit of the easiest explanation on the *old system*" (*loc. cit.* p. 563).

The fact that Priestley was the first to consciously isolate oxygen is not contested by M. Berthelot, although he is careful to point out, what is not denied, that the exact date of the discovery depends on Priestley's own statement, and that his first publication of it was made in a work published in London in 1775. It was known before Priestley's famous experiment that the red oxide of mercury, originally formed by heating the metal in contact with air, would again yield mercury by the simple action of heat and without the intervention of any reducing agent. Bayen, six months before the date of Priestley's discovery, had noted that a gas was thus disengaged, but he gave no description of its nature, contenting himself merely by pointing out the analogy which his observations appeared to possess to those of Lavoisier on the existence of an elastic fluid in certain substances. Afterwards, when the facts were established, Bayen drew attention to his earlier experiments and claimed, not only the discovery of oxygen, but all that Lavoisier deduced from it. "But," says M. Berthelot, in reference to this circumstance, "his contemporaries paid little heed to his pretensions, nor will posterity pay more" (*La Révolution Chimique*, p. 60).

M. Berthelot, however, does not dismiss Lavoisier's claims to a participation in the discovery in the same summary fashion. On the contrary, whilst not explicitly claiming for him the actual isolation, in the first instance, of oxygen, the whole tenor of his argument is to palliate, and even to justify, his demand to be regarded as an independent discoverer of the gas. He begins by asserting that Lavoisier had already a presentiment of its existence in 1774, and he quotes, in support of this assumption, an abstract from Lavoisier's memoir, pub-

lished in December 1774, in the *Journal de Physique* of the Abbé Rozier: "This air, deprived of its fixable portion (by metals during calcination), is in some fashion decomposed, and this experiment would seem to afford a method of analysing the fluid which constitutes our atmosphere and of examining the principles of which it is composed. . . . I believe I am in a position to affirm that the air, as pure as it is possible to suppose it, free from moisture and from every foreign substance, far from being a simple body, or element, as is commonly thought, should be placed, on the contrary, . . . in the group of the mixtures, and perhaps even in that of the compounds."

M. Berthelot further asserts that Lavoisier was at this time the first to recognise the true character of air, and he expresses his belief that it is probable that he would himself have succeeded in isolating its constituents if the path of inquiry had been left to him alone. It is no disparagement to Lavoisier's prescience to say that there is nothing in these lines, nor in the memoir which deals with the repetition of Boyle's experiments on the calcination of tin to which they refer, to show that Lavoisier had made any advance beyond the position of Hooke and Mayow. It has been more than once pointed out that the chemists of the seventeenth century understood the true nature of combustion in air much better than their brethren of the last quarter of the eighteenth century. Hooke, in the *Micrographia*, and Mayow, in his *Opera Omnia Medicophysica*, indicated that combustion consists in the union of something with the body which is being burnt; and Mayow, both by experiment and inference, demonstrated in the clearest way the analogy between respiration and combustion, and showed that in both processes one constituent

only of the air is concerned. He distinctly stated that not only is there increase of weight attending the calcination of metals, but that this increase is due to the absorption of the same *spiritus* from the air that is necessary to respiration and combustion. Mayow's experiments are so precise, and his facts so incontestable, that, as Chevreul has said, it is surprising that the truth was not fully recognised until a century after his researches. (*Vide* Watts' *Dictionary of Chemistry*, by Morley and Muir ; art. "Combustion," p. 242.)

It is now necessary to examine Lavoisier's claims rather more closely and in the light of M. Berthelot's book. A *résumé* of his work *On the Calcination of Tin* was given by Lavoisier to the Academy in November 1774, but the complete memoir was not deposited until May 1777. A careful comparison of an abstract of what was stated to the Academy in November 1774, contributed by Lavoisier himself, in December 1774, to the *Journal de Physique* of the Abbé Rozier, makes it evident that very substantial additions were made to the communication before it was finally printed in the *Mémoires de l'Académie des Sciences.* The possibility of this is allowed by M. Berthelot. He says (p. 58) : "A summary communication, often given *viva voce* to a learned society, such as the Academy of Sciences of Paris or the Royal Society of London, would immediately call forth verifications, ideas, and new experiments, which would develop the range and even the results of such communication. The original author, when printing his memoir, would in return—and for this he is hardly blamable—embody these additional results and later interpretations. It thus becomes most difficult to assign impartially to each his share in a rapid succession of discoveries" (*loc. cit.* p. 58).

But although, as we shall see, Lavoisier was certainly aware of Priestley's great discovery, no allusion is made to the gas, nor to Priestley's previous work on the other constituent of air, which is printed in the *Philosophical Transactions* for 1772, and for which he was awarded the Copley Medal by the Royal Society. It is simply impossible to believe that Lavoisier could have been uninfluenced by this work. Indeed, we venture to assert that the full and clear recognition of the non-elementary nature of air which he eventually made was based upon it. It is noteworthy that in the early part of his memoir he states his opinion that the addition not only of powdered charcoal, but of any phlogistic substance to a metallic calx is attended with the formation of fixed air. It is certain that at this period he had not only not consciously obtained any gas resembling Priestley's dephlogisticated air from any calx with which he had experimented, but that none of his experiments had afforded him any idea that the gas absorbed during calcination was identical with it.

At Easter 1775 Lavoisier presented a memoir to the Academy "On the Nature of the Principle which Combines with Metals during Calcination." This was "*relu le* 8 *août*, 1778." To the memoir there is a note stating that the first experiments detailed in it were performed more than a year before; those on the red precipitate were made by means of *a burning glass* in the month of November 1774, and were repeated in the spring of 1775 at Montigny in conjunction with M. Trudaine. In this paper Lavoisier first distinctly announces that the principle which unites with metals during their calcination, which increases their weight, and which transforms them into calces, is nothing else "than the purest and most salubrious part of the air; so that if

that air which has been fixed in a metallic combination again becomes free, it reappears in a condition in which it is eminently respirable, and better adapted than the air of the atmosphere to support inflammation and the combustion of substances" (*Œuvres de Lavoisier*, official edition, vol. ii. p. 123). He then describes the method of preparing oxygen by heating the red oxide of mercury, and compares the properties of the gas with those of fixed air. There is, however, no mention of Priestley, nor any reference to his experiments. It can hardly be doubted that in this memoir Lavoisier intended his readers to believe that he was "the true and first discoverer" of the gas which he afterwards named oxygen. This is borne out by certain passages in his subsequent memoir "On the Existence of Air in Nitrous Acid"; "*lu le* 20 *avril*, 1776, *remis en décembre* 1777." He had occasion incidentally to prepare the red oxide of mercury by calcining the nitrate, and says that he obtained from it a large quantity of an air "much purer than common air, in which candles burnt with a much larger, broader, and more brilliant flame, and which in no one of its properties differed from that which I had obtained from the calx of mercury, known as *mercurius precipitatus per se*, and which Mr. Priestley had procured from a great number of substances by treating them with nitric acid."

In another part of this memoir he says that "perhaps, strictly speaking, there is nothing in it of which Mr. Priestley would not be able to claim the original idea; but as the same facts have conducted us to diametrically opposite results, I trust that, if I am reproached for having borrowed my proofs from the works of this celebrated philosopher, my right at least to the conclusions will not be contested." M. Berthelot remarks on the irony of this passage: we may infer from it that

the friends of the English chemist had not been altogether idle. In his memoir "On the Respiration of Animals," read to the Academy in 1777, he again appears to admit the claim of Priestley to at least a share in the discovery: "It is known from Mr. Priestley's and my experiments that *mercurius precipitatus per se* is nothing but a combination," etc. In several subsequent communications Priestley's name is mentioned in very much the same connection, until we come to the classical memoir "On the Nature of the Acids," when it is said: "I shall henceforth designate the dephlogisticated air, or the eminently respirable air . . . by the name of the *acidifying principle*, or, if it is preferred to have the same signification under a Greek word, by that of the '*principe oxygine.*'"

In none of the memoirs after that of Easter 1775 is the claim for participation more than implied; it is made explicitly for the first time in the paper "On a Method of Increasing the Action of Fire," printed in the *Mémoires de l'Académie* for 1782, and in these words: "It will be remembered that at the meeting of Easter 1775 I announced the discovery, which I had made some months before with M. Trudaine,[1] in the laboratory at Montigny, of a new kind of air, up to then absolutely unknown, and which we obtained by the reduction of *mercurius precipitatus per se.* This air, which Mr. Priestley discovered at very nearly the same time as I, and I believe even before me, and which he had procured mainly from the combination of minium and of several other substances with nitric acid, has been named by him *dephlogisticated air.*"

In the "Traité Elémentaire de Chimie" the claim for participation is again asserted in these words:

[1] M. Trudaine de Montigny died in 1777.

"This air, which Mr. Priestley, Mr. Scheele, and I discovered at about the same time." . . .

Now there is no question that Lavoisier knew of the existence of oxygen some months before he made the experiments with the burning glass of M. Trudaine at Montigny, *for the simple reason that Priestley had already told him of it.* Priestley left Leeds in 1773 to become the librarian and literary companion of Lord Shelburne, and in the autumn of 1774 he accompanied his lordship to the Continent, and spent the month of October in Paris. Lavoisier was famous for his hospitality; his dinners were celebrated; and Priestley, in common with every foreign *savant* of note who visited Paris at that period, was a welcome guest. What followed is best told in Priestley's own words: "Having made the discovery [of oxygen] some time before I was in Paris, in the year 1774, I mentioned it at the table of Mr. Lavoisier, when most of the philosophical people of the city were present, saying that it was a kind of air in which a candle burnt much better than in common air, but I had not then given it any name. At this all the company, and Mr. and Mrs. Lavoisier as much as any, expressed great surprise. I told them I had gotten it from *precipitate per se* and also from *red lead.* Speaking French very imperfectly, and being little acquainted with the terms of chemistry, I said *plombe rouge*, which was not understood till Mr. Macquer said I must mean *minium.*"

In his account of his own work on dephlogisticated air, given in his *Observations*, etc., 1790 edition, he further says, vol. ii. p. 108: "Being in Paris on the October following [the August of 1774], and knowing that there were several very eminent chemists in that place, I did not omit the opportunity, by means of my

friend Mr. Magellan,[1] to get an ounce of *mercurius calcinatus* prepared by Mr. Cadet, of the genuineness of which there could not possibly be any suspicion; and, at the same time, I frequently mentioned my surprise at the kind of air which I had got from this preparation to Mr. Lavoisier, Mr. Le Roy, and several other philosophers, who honoured me with their notice in that city, and who, I daresay, cannot fail to recollect the circumstance."

If any further evidence is required to prove that Lavoisier was not only not "the true and first discoverer" of oxygen, but that he has absolutely no claim to be regarded even as a later and independent discoverer, it is supplied by M. Berthelot himself. Not the least valuable portion of M. Berthelot's book, as an historical work, is that which he devotes to the analysis of the thirteen laboratory journals of Lavoisier, which have been deposited, by the pious care of M. de Chazelles, his heir, in the archives of the Institute. M. Berthelot has given us a synopsis of the contents of almost every page of these journals, with explanatory remarks, and dates when these could be ascertained. As he well says, these journals "are of great interest because they inform us of Lavoisier's methods of work and of the direction of his mind—I mean the successive steps in the evolution of his private thought." On the fly-leaf of the third journal is written, "*du* 23 *mars*, 1774, *au* 13 *février*, 1776." From p. 30 we glean that Lavoisier visited his friend M. Trudaine at Montigny about ten days after his conversation with Priestley, and repeated the latter's experiments on the marine

[1] Prof. Grimaux (*Lavoisier*, p. 51), says: "Un de ses [Lavoisier's] amis qui habitait Londres, Magalhaens ou Magellan, de la famille du célèbre navigateur, lui envoyait tous les mémoires sur les sciences qui paraissaient en Angleterre et le tenait au courant des découvertes de Priestley."

acid and alkaline airs (hydrochloric acid gas and ammonia). He is again at Montigny some time between February 28 and March 31, 1775, and repeats not only Priestley's experiments on the decomposition of mercuric oxide, presumably by means of M. Trudaine's famous burning glass, but also his observations on the character of the gas. The fly-leaf of the fourth journal informs us that it extends from February 13, 1776, to March 3, 1778. On p. 1 is an account of experiments made February 13, on "précipité *per se* de chez M. Baumé," in which the disengaged gas is spoken of as "*l'air déphlogistique de M. Prisley*" (*sic*). Such a phrase in a private note-book is absolutely inconsistent with the idea that at this time Lavoisier considered himself as an independent discoverer of the gas. How he came to regard himself as such we need not inquire. Nor is it necessary to occupy your time by any examination of the arguments by which M. Berthelot, with the skill of a practised advocate, would seem to identify himself with the case of his client. We would do him the justice of recognising the difficulty of his position. He seeks to discharge an obligation, of which the acknowledgment has been too long delayed. The Académie des Sciences a year ago awoke to the sense of its debt of gratitude to the memory of the man who had laboured so zealously for its honour, and even for its existence, during the stormy period of which France has just celebrated the centenary, and out of the *éloge* on Lavoisier which M. Berthelot, as Perpetual Secretary, was commissioned to deliver, has grown *La Révolution Chimique*. To write eulogy, however, is not necessarily to write history. We cannot but think that M. Berthelot has been hampered by his position, and that his opinion, or at least the free expression of it, has been

fettered by the conditions under which he has written. We imagine we discern between the lines the consciousness that, to use Brougham's phrase, the brightness of the illustrious career which he eulogises is dimmed with spots which a regard for historical truth will not permit him wholly to ignore.

Two cardinal facts made the downfall of phlogiston complete—the discovery of oxygen and the determination of the compound nature of water. M. Berthelot's contention is that not only did Lavoisier effect the overthrow, but he also discovered the facts. In other words, he has not only a claim to a participation in the discovery of oxygen, but he is also "the true and first discoverer" of the non-elementary nature of water. This second claim is directly and explicitly stated. Although it is supported by a certain ingenuity of argument, we venture to think that we shall be able to show it has no greater foundation in reality than the first.

Members of the British Association who are at all familiar with its history, will recall the fact that this is not the first occasion on which the attempt to transfer "those laurels which both time and truth have fixed upon the brow of Cavendish" has had to be resisted. At the Birmingham Meeting of 1839 the Rev. W. Vernon Harcourt, who then presided, devoted a large portion of his address to an able and eloquent vindication of Cavendish's rights. The attack came then as now from the Perpetual Secretary of the French Academy, and the charges were also formulated then, as now, in an *éloge* read before that learned body. The assailant was M. Arago, who did battle, not for his countryman Lavoisier, whose claims are dismissed as "pretensions," but on behalf of James Watt, the great

engineer, who was one of the foreign members of the Institute.

It is not my wish to trouble you at any length with the details of what has come to be known in the history of scientific discovery as the Water Controversy—a controversy which has exercised the minds and pens of Harcourt, Whewell, Peacock, and Brougham in England; of Brewster, Jeffrey, Muirhead, and Wilson in Scotland; of Kopp in Germany; and of Arago and Dumas in France. This controversy, it has been said, takes its place in the history of science side by side with the discussion between Newton and Leibnitz concerning the invention of the Differential Calculus, and that between the friends of Adams and Leverrier in reference to the discovery of the planet Neptune. Up to now it has practically turned upon the relative merits of Cavendish and Watt. M. Berthelot is the first French *savant* of any note who has seriously put forward the claims of Lavoisier, his countryman and predecessor Dumas having deliberately rejected them.

At the risk of wearying you with detail, I am under the necessity of restating the facts in order to make the position clear. Some time before April 18, 1781, Priestley made what he called "a random experiment" for the entertainment of a few philosophical friends. It consisted in exploding a mixture of inflammable air (presumably hydrogen) and common air, contained in a closed glass vessel, by the electric spark, in the manner first practised by Volta in 1776. The experiment was witnessed by Mr. John Warltire, a lecturer on natural philosophy and a friend of Priestley, who had rendered him the signal service of giving him the sample of the mercuric oxide from which he had first obtained oxygen. Warltire drew Priestley's attention

to the fact that after the explosion the sides of the glass vessel were bedewed with moisture. Neither of the experimenters attached any importance to the circumstance at the time, Priestley being of opinion that the moisture was pre-existent in the gases, as no special pains were taken to dry them. Warltire, however, conceived the notion that the experiment would afford the means of determining whether heat was ponderable or not, and hence he was led to repeat it, firing the mixture in a copper vessel for greater safety. The results of these observations are contained in Priestley's *Experiments and Observations on Air*, vol. v., 1781, App. p. 395.

At this period Cavendish was engaged on a series of experiments "made, as he says, principally with a view to find out the cause of the diminution which common air is well known to suffer by all the various ways in which it is phlogisticated, and to discover what becomes of the air thus lost or condensed" (Cavendish, *Philosophical Transactions*, 1784, p. 119). On the publication of Priestley's work he repeated Warltire's experiment, for, he says, as it "seemed likely to throw great light on the subject I had in view, I thought it well worth examining more closely." The series of experiments which Cavendish was thus induced to make, and which he made with all his wonted skill in quantitative work, led him at some time in the summer of 1781 to the discovery that a mixture of two volumes of the inflammable air from metals (the gas we now call hydrogen) with one volume of the dephlogisticated air of Priestley combine together under the influence of the electric spark, or by burning, to form the same weight of water. If Cavendish had published the results of these observations at or near the time he

obtained them, there would have been no Water Controversy. But in the course of the trials he found that the condensed water was sometimes acid, and the search for the cause of the acidity (which incidentally led to the discovery of the composition of nitric acid) occasioned the delay. The main result that a mixture of two volumes of inflammable air and one volume of dephlogisticated air could be converted into the same weight of water was, however, communicated to Priestley, as he relates in a paper in the *Philosophical Transactions* for 1783. Priestley was at this time interested in an investigation on the seeming convertibility of water into air, and he was led to repeat Cavendish's experiments, some time in March 1783, on what was apparently the converse problem. Priestley, however, made a fatal blunder in the repetition. With the praiseworthy idea of obviating the possibility of any moisture in the gases, he prepared the dephlogisticated air from nitre, and the inflammable air by heating what he calls "perfectly made charcoal" in an earthenware retort. At this period, it must be remembered, there was no sharp distinction between the various kinds of inflammable air: hydrogen, sulphuretted hydrogen, marsh gas and olefiant gas, coal gas, the vapours of ether and turpentine, and the gas from heated charcoal—consisting of a mixture of carbonic oxide, marsh gas, and carbonic acid—were indifferently termed "inflammable air." Priestley attempted to verify Cavendish's conclusion on the identity of the weight of the gases used with that of the water formed; but his method in this respect, as in his choice of the inflammable air, was wholly defective, and could not possibly have given him accurate results. It consisted in wiping out the water from the explosion vessel by

means of a weighed piece of blotting-paper, and determining the increase of weight of the paper. He says, however: "I always found as near as I could judge the weight of the decomposed air in the moisture acquired by the paper. . . . I wished, however, to have had a nicer balance for this purpose; the result was such as to afford a strong presumption that the air was reconverted into water, and therefore that the origin of it had been water." These results, together with those on the conversion of water into air, were communicated towards the end of March 1783 by Priestley to Watt, who began to theorise upon them, and then to put his thoughts together in the form of a letter to Priestley, dated April 26, 1783, and which he requested might be read to the Royal Society on the occasion of the presentation of Priestley's memoir. In this letter Watt says: "Let us now consider what obviously happens in the case of the deflagration of the inflammable and dephlogisticated air. These two kinds of air unite with violence, they become red-hot, and upon cooling totally disappear. When the vessel is cooled, a quantity of water is found in it equal to the weight of the air employed. This water is then the only remaining product of the process, and *water*, *light*, and *heat* are all the products. *Are we not then authorised to conclude that water is composed of dephlogisticated air and phlogiston deprived of part of their latent or elementary heat; that dephlogisticated or pure air is composed of water deprived of its phlogiston and united to elementary heat and light, etc.?*"

This letter, although shown to several Fellows of the Society, was not publicly read at the time intended. Priestley, before its receipt, had detected the fallacy of his experiments on the seeming conversion of water into air, and as much of the letter was concerned with this

matter, Watt requested that it should be withdrawn. Watt, however, as he tells Black[1] in a letter dated June 23, 1783, had not given up his theory as to the nature of water, and on November 26, 1783, he restated his views more fully in a letter to De Luc. In the meantime Cavendish, having completed one section of his investigation, sent in a memoir to the Royal Society, which was read on January 15, 1784, in which he gives an account of his experiments, and announces his conclusion "that dephlogisticated air is in reality nothing but dephlogisticated water, or water deprived of its phlogiston; or, in other words, that water consists of dephlogisticated air united to phlogiston; and that inflammable air is either pure phlogiston, as Dr. Priestley and Mr. Kirwan suppose, or else water united to phlogiston." Watt thereupon requested that his letter to De Luc should be published, and it was accordingly read to the Royal Society on April 29, 1784. Which of the two—Cavendish or Watt—is, under these circumstances, to be considered as "the true and first discoverer" of the compound nature of water is the question which has been hitherto the main subject of the Water Controversy.

Let us now consider the matter as it affects Lavoisier. In 1783 Lavoisier had publicly declared against the doctrine of phlogiston, or rather, as M. Dumas puts it, "against the crowd of entities of that name which had no quality in common except that of being intangible by every known method" (*Leçons sur la Philosophie Chimique*, p. 161). How completely Lavoisier had dissociated himself from the theory may be gleaned from his memoir of that year. "Chemists," he says, "have made a vague principle of phlogiston which is not

[1] Watt, *Correspondence*, p. 31.

strictly defined, and which in consequence accommodates itself to every explanation into which it is pressed. Sometimes this principle is heavy and sometimes it is not; sometimes it is free fire, and sometimes it is fire combined with the earthy element; sometimes it passes through the pores of vessels, and sometimes they are impenetrable to it: it explains at once causticity and non-causticity, transparency and opacity, colours and the absence of colours. It is a veritable Proteus which changes its form every moment."

But in reality Lavoisier had merely renounced one fetich for another. At the time that he penned these lines he was as much under the thraldom of *le principe oxygine* as the most devoted follower of Stahl was in the bondage of phlogiston. That the calcination of metals was but a slow combustion had been fully recognised. M. Berthelot tells us that as far back as the March of 1774 Lavoisier had written in his laboratory journal: "I am persuaded that the inflammation of inflammable air is nothing but a fixation of a portion of the atmospheric air, a decomposition of air. . . . In that case in every inflammation of air there ought to be an increase of weight," and he tried to ascertain this by burning hydrogen at the mouth of a vessel from which it was being disengaged. In the following year he asks, What remains when inflammable air is burnt completely? According to the theory by which he is now swayed it should be an acid, and he made many attempts to capture this acid. In 1777 he and Bucquet burnt six pints of the inflammable air from metals in a bottle containing lime-water, in the expectation that fixed air would be the result. And in 1781 he repeated the experiment with Gengembre, with the modification that the oxygen was caused to burn in an atmosphere of

hydrogen, but not a trace of any acid product could be detected. Of course there must have been considerable quantities of water formed in these experiments, but Lavoisier was preoccupied with the conviction that oxidation meant acidification, and the presence of the water was unnoticed, or, if noticed, was unheeded. Macquer, in 1776, had drawn attention to the formation of water during the combustion of hydrogen in air, but Lavoisier has stated that he was ignorant of that observation. What was it then that put him on the right track? We venture to think that M. Berthelot has himself supplied the answer. He says (p. 114): "Rumours of Cavendish's trials had spread throughout the scientific world during the spring of 1783. . . . Lavoisier, always on the alert as to the nature of the products of the combustion of hydrogen, was now in such position that the slightest hint would enable him to comprehend its true nature. He hastened to repeat his trials, as he had the right to do, never having ceased to occupy himself with a question which lay at the very heart of his doctrine."

"On the 24th of June 1783," continues M. Berthelot, "he repeated the combustion of hydrogen in oxygen, and he obtained a notable quantity of water without any other product, and he concluded from the conditions under which he had worked that the weight of the water formed could not be other than equal to that of the two gases which had formed it. The experiment was made in the presence of several men of science, among whom was Blagden, a member of the Royal Society of London, who on *this occasion* recalled the observations of Cavendish (*qui rappela à cette occasion les observations de Cavendish*)."

On the following day Lavoisier published his results.

The following is the official minute of the communication, translated from the register of the sittings of the Académie des Sciences :—

Meeting of Wednesday, 25th June 1783.

MM. Lavoisier and De Laplace announced that they had lately repeated the combustion of Combustible Air with Dephlogisticated Air; they worked with about 60 *pints of the airs, and the combustion was made in a closed vessel: the result was very pure water.*

The cautious scribe who penned that minute did not commit himself too for. M. Berthelot, however, regards it as the first certain date of publication, established by authentic documents, in the history of the discovery of the composition of water; "a discovery," he adds, "which, on account of its importance, has excited the keenest discussion."

You will search in vain through the laboratory journals, as given by M. Berthelot, for any indications either of experiments or reflections which would enable you to trace the course of thought by which Lavoisier was guided to the truth. There is absolutely nothing on the subject until in the eighth volume (25 *mars* 1783, *au février* 1784), and on p. 63 we come to the experiment of 24th June, and we read: "In presence of Messieurs Blagden, of [name illegible], de Laplace, Vandermonde, de Fourcroy, Meusnier, and Legendre, we have combined in a bell-jar dephlogisticated air and inflammable air drawn from iron by means of sulphuric acid, etc. . . . The amount of water may be estimated at 3 drachms: the amount which should have been obtained was 1 ounce 1 drachm and 12 grains. Thus we must suppose that there was a loss of two-thirds of the amount of the air, or that there has been a loss of weight."

And this is the experiment which, according to M. Berthelot, enabled Lavoisier to conclude that "the weight of the water formed could not be other than equal to that of the two gases which had formed it"! It is on this single experiment, hurriedly and imperfectly done, that Lavoisier's claim to the discovery of the compound nature of water is based. M. Berthelot objects to the assumption that it was hurriedly done. He says, on p. 114: "Lavoisier caused a new apparatus to be made, with a couple of tubes and two reservoirs for the gases; an arrangement which would require a certain amount of time to put together; this circumstance proves that it could not have been an improvised trial." To what extent it was improvised will be seen immediately.

Now although the laboratory journals do not in this case "inform us of Lavoisier's methods, and of the direction of his mind . . . the successive steps in the evolution of his private thought," we have other means of ascertaining how he arrived at his knowledge. The method was simplicity itself: he *was told of the fact, and his informant was none other than Cavendish's assistant, Blagden.*

Cavendish's memoir was published in 1784. Before it was struck off its author caused the following addition to be made: "During the last summer also a friend of mine gave some account of them [the experiments] to M. Lavoisier, as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of phlogiston; but at that time so far was M. Lavoisier from thinking any such opinion warranted that, till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water."

This addition, as I have had the opportunity of verifying by an inspection of the original MSS. in the archives of the Royal Society, was made in the handwriting of Cavendish's assistant and amanuensis, Blagden.

When Lavoisier's memoir appeared it was found to contain the following reference to this circumstance: "It was on the 24th of June that M. de Laplace and I made this experiment in presence of MM. le Roi, Vandermonde, and several other Academicians, and of Mr. Blagden, the present Secretary of the Royal Society of London. The latter informed us (*ce dernier nous apprit*) that Mr. Cavendish had already tried, in London, to burn inflammable air in closed vessels, and that he had obtained a very sensible quantity of water."

This reference was so partial, and its meaning so ambiguous, that Blagden addressed the following letter to Crell, to be published in his *Chemische Annalen* (Crell's *Annalen*, 1786, vol. i. p. 58).

It is so direct and conclusive that I offer no apology for giving it almost entire:[1]—

I can certainly give you the best account of the little dispute about the first discoverer of the artificial generation of water, as I was the principal instrument through which the first news of the discovery that had been already made was communicated to Mr. Lavoisier. The following is a short statement of the history:—

In the spring of 1783 Mr. Cavendish communicated to me, and other members of the Royal Society, his particular friends, the result of some experiments with which he had for a long time been occupied. He showed us that out of them he must draw the conclusion that dephlogisticated air was nothing else than water deprived of its phlogiston; and, *vice versa*, that water was dephlogisticated air united with phlogiston. About the same time the news was brought to London that Mr. Watt, of Birmingham, had

[1] Mr. Muirhead's translation. *Vide* Watt, *Correspondence, Composition of Water*, p. 71.

been induced by some observations to form a similar opinion. Soon after this I went to Paris, and in the company of Mr. Lavoisier and of some other members of the Royal Academy of Sciences I gave some account of these new experiments and of the opinions founded upon them. They replied that they had already heard something of these experiments, and particularly that Dr. Priestley had repeated them. They did not doubt that in such manner a considerable quantity of water might be obtained, but they felt convinced that it did not come near to the weight of the two species of air employed, on which account it was not to be regarded as water formed or produced out of the two kinds of air, but was already contained in and united with the airs, and deposited in their combustion. This opinion was held by Mr. Lavoisier, as well as by the rest of the gentlemen who conferred on the subject; but, as the experiment itself appeared to them very remarkable in all points of view, they unanimously requested Mr. Lavoisier, who possessed all the necessary preparations, to repeat the experiment, on a somewhat larger scale, as early as possible. This desire he complied with on the 24th June 1783 (as he relates in the latest volume of the Paris memoirs). From Mr. Lavoisier's own account of his experiment, it sufficiently appears that at that period he had not yet formed the opinion that water was composed of dephlogisticated and inflammable airs, for he expected that a sort of acid would be produced by their union. In general, Mr. Lavoisier cannot be convicted of having advanced anything contrary to truth; but it can still less be denied that he concealed a part of the truth; for he should have acknowledged that I had, some days before, apprised him of Mr. Cavendish's experiments, instead of which the expression "il nous apprit" gives rise to the idea that I had not informed him earlier than that very day. In like manner Mr. Lavoisier has passed over a very remarkable circumstance—namely, that the experiment was made in consequence of what I had informed him of. He should likewise have stated in his publication not only that Mr. Cavendish had obtained "une quantité d'eau très sensible," but that the water was equal to the weight of the two airs added together. Moreover, he should have added that I had made him acquainted with Messrs. Cavendish and Watt's conclusions—namely, that water, and not an acid, or any other substance, arose from the combustion of the inflammable and dephlogisticated airs. But *those* conclusions

opened the way to M. Lavoisier's present theory, which perfectly agrees with that of Mr. Cavendish, only that Mr. Lavoisier accommodates it to his old theory, which banishes phlogiston. . . . The course of all this history will clearly convince you that Mr. Lavoisier (instead of being led to the discovery by following up the experiments which he and Mr. Bucquet had commenced in 1777) was induced to institute again such experiments, solely by the account he received from me, and of our English experiments; and that he really discovered nothing but what had before been pointed out to him to have been previously made out and demonstrated in England.

To this letter, reflecting so gravely on his honour and integrity, Lavoisier made no reply. Nor did Laplace, Le Roi, Vandermonde, or any one of the Academicians concerned, vouchsafe any explanation. *De non apparentibus et de non existentibus eadem ratio.* No explanation appeared, because none was possible. M. Berthelot ignores this letter, which is the more remarkable, since reference is made to it in more than one of the publications which he tells us he has consulted in the preparation of his account of the Water Controversy. If he knew of it, he must regard it either as unworthy of an answer or as unanswerable.

It would be heaping Ossa on Pelion to adduce further evidence from letters of the time of what Lavoisier's contemporaries thought of his claims. *De mortuis nil nisi bonum.* I would much more willingly have dwelt upon the virtues of Lavoisier, and have let his faults lie gently on him; but I have felt it incumbent on me on this occasion to make some public answer to M. Berthelot's book, and in no place could that answer be more fittingly given than in this town, which saw the dawn of that work out of which these grand discoveries arose. It may be that much of what I have had to say is as a twice-told tale to many of

you. I trust I need make no apology on that account. The honour of our ancestors is in our keeping, and we should be unworthy of our heritage and false to our trust if we were slow to resent or slack to repel any attempt to rob them of that glory which is their just right and our proud boast.

Addendum

Translations of the foregoing Address appeared in several French periodicals devoted to popular science, accompanied by criticisms, for the most part hostile. The *Revue Scientifique* of 25th October 1890 also published the Address, to which, on the invitation of the editor, M. Charles Richet, M. Berthelot prefixed a letter, which may be translated as follows :—

I have no direct concern in the republication of Mr. Thorpe's address which you purpose making in the *Revue*. Personally, I have not any reason to complain of his courtesy, and I should have been silent so far as he is concerned, holding that one is not bound to enter into a controversy which is purely critical, where no new fact is alleged, and where the judgment of public opinion suffices to set things in their true place; however, I comply with your request to let your readers know what my opinion is.

To my mind, nothing is more opposed to truth and justice than the introduction of national prejudices into the history of science. All civilised nations are at one in proclaiming the glory of Newton, the greatest of astronomers, and yet the majority of English men of science, refusing to treat his rivals with equity, are not agreed to recognise Leibnitz's rights to the invention of the differential calculus: they are as prejudiced in this respect as was Newton himself. Something analogous occurs in regard to the discoveries which created modern chemistry a hundred years ago.

Unquestionably, Priestley and Cavendish are recognised by all as great discoverers. I have myself taken pains to describe Priestley's discovery of the principal gases in terms of admiration (*La Révolution Chimique*, p. 39), and especially that of oxygen, which

I unreservedly attribute to him (pp. 61, 62). I have also detailed, with the encomiums which they merit, the investigations of Cavendish, "one of the most powerful scientific minds of the last century," and particularly his fruitful research on (to use Blagden's phrase) the artificial generation of water. But the well-merited praise accorded to these English *savants* does not prevent some of their countrymen from persistently denying the right of Lavoisier to the discovery and co-ordination of those general ideas on which rests our actual conception of matter, more especially in relation to the composition of air and water. This, I venture to repeat, is an incident in the long-standing feud, continually being renewed in the history of science, between the sagacious discoverers of particular facts and the men of genius who frame general theories. The opinion of most Continental men of science seems, however, to be decided on this special point, as may be seen from the judgment given, not only by Dumas, but by Hoefer in his *History of Chemistry*, by H. Kopp in his careful account of the discovery of the composition of water, and by many others. I have merely concurred with them.

It was in this spirit that I sought to trace the history of the discoveries which constituted the doctrine of modern chemistry, by faithfully reproducing all its phases, whilst at the same time indicating the continuity of sequence in the facts and the paternity of the ideas. I did this with an impartiality which has brought upon me the reproach that I have been indifferent to the reputation of my countrymen—the very opposite to the accusations which are now directed against me.

As regards the composition of air, it is easy to separate facts from ideas. It is certain that the discovery of oxygen is due to Priestley. But, said Lavoisier, "If I am reproached for having borrowed my proofs from the works of this celebrated philosopher, at least none will contest my right to the conclusions, which are often diametrically opposed to his."

Priestley, obstinately adhering to the theory of phlogiston, regarded his new gas as consisting of the very substance of air deprived simply of its phlogiston; whilst nitrogen, according to him, was formed also of this same substance combined with a complementary portion of phlogiston. He remained faithful to this doctrine, which obscured the true nature of the greater number of chemical phenomena, until the moment when, like Lavoisier,

persecuted by his countrymen, who now proclaim his fame, driven from home, his laboratory burnt by a mob, and threatened with death, he fled to America, where he died in sadness and in solitude. Even more unfortunate was Lavoisier!

But whatever may have been the personal fate of these two great men, if it is true that Priestley discovered oxygen, it is not the less certain that the true theory of the nature of air is due to Lavoisier.

The history of the composition of water is more complicated. In reality the discovery of the facts belongs neither wholly to Cavendish—who undoubtedly played a most important part, inasmuch as he gave the impetus towards the definitive solution—nor to Lavoisier, who first established a knowledge of the facts by his public experiments and his published writings—nor even to the two combined. They had predecessors, and at the moment even when the light came, Monge played an essential part in the rigorous demonstration of which Mr. Thorpe apparently has no suspicion. Thus each man's share in this history cannot be settled by a word: we require to follow exactly the gradual progress of experiment and publication. But here, again, if Lavoisier is not the principal discoverer of the facts, it is he who has the incontestable merit of having furnished the exact interpretation of the phenomena, freed from the mists of phlogiston, to which Cavendish seems to have remained faithful to the day of his death.

I have elsewhere laid bare all these facts, and I have no intention of reproducing here the details of a controversy exhausted even in Lavoisier's time, and in which Mr. Thorpe does no more than reproduce the unjustifiable imputations of Blagden, who, impelled by passion, went so far as to interpolate and falsify, with his own hand, the manuscript memoirs of Cavendish, in order to gain arguments in support of his accusations.

Moreover, nothing more decisively establishes the part played by Lavoisier, and his right to the institution of our modern theories, than the letter of a contemporary English *savant*, Black, as celebrated for his discoveries in physics as in chemistry, and who might have put forward claims on his own account. In 1791 he wrote to Lavoisier, in a letter equally honourable to both:—"The numerous experiments which you have made on a large scale, and which you have so well devised, have been pursued with so much care and with such scrupulous attention to details, that nothing can

be more satisfactory than the proofs you have obtained. The system which you have based on the facts is so intimately connected with them, is so simple and so intelligible, that it must become more and more generally approved and adopted by a great number of chemists who have long been accustomed to the old system. . . . Having for thirty years believed and taught the doctrine of phlogiston as it was understood before the discovery of your system, I, for a long time, felt inimical to the new system, which represented as absurd that which I had hitherto regarded as sound doctrine; but this enmity, which springs only from the force of habit, has gradually diminished, subdued by the clearness of your proofs and the soundness of your plan."

We can but hope to see the day when the scientific men of England will conform to the opinion of one of the most illustrious of their countrymen. M. BERTHELOT, of the Institute.

Certain passages in M. Berthelot's letter, more especially those reflecting on the character of Blagden, seemed to me to require some notice. I therefore addressed the following remarks to *Nature* of 6th November 1890 :—

I quite agree with M. Berthelot that nothing is more opposed to truth and justice than the introduction of national prejudices into the history of science. It was for that reason that I felt compelled, in the Leeds address, to protest against the spirit and bias of the accounts of the discovery of the facts relating to oxygen and the composition of water given in *La Révolution Chimique*. Although M. Berthelot's letter somewhat confuses the issues, there is, in reality, but small difference between us. What I ventured to criticise was the general tone and tendency of M. Berthelot's argument, which seems to palliate, and even to justify, Lavoisier's pretensions to a discovery in which he has no right even to be considered as a participator. M. Berthelot now tells us in his letter that he attributes

the discovery of oxygen unreservedly to Priestley. So far so good. It is something gained to have thus secured such an unqualified statement from one who occupies the position of authority in the world of chemistry in France that is enjoyed by the present Perpetual Secretary of the Academy. We may well hope, therefore, that this particular question has been finally set at rest.

M. Berthelot need not ask British men of science to conform to the opinion of Black. They already do so. That to Lavoisier, and to Lavoisier alone, belongs the merit of having effected the overthrow of the theory of phlogiston, and of having to that extent laid the foundation of modern chemistry, is not questioned on this side of the Channel. So far as I know, it has only been among Lavoisier's own countrymen that any doubt on this point has been raised. We all remember the passionate scorn with which Lavoisier repudiated and protested against the attempts of his compatriots to rob him of his rights: "Cette théorie n'est donc pas comme je l'entends dire — la théorie des chimistes français; elle est la *mienne*, et c'est une propriété que je réclame auprès de mes contemporains et de la postérité." It is true, as M. Berthelot implies, that Black has claims. Lavoisier himself admits as much. It would be easy, if it were not beside the points at issue, to match the letter which M. Berthelot quotes by others from Lavoisier in which he ascribes to Black the germs of his doctrine. M. Berthelot, I repeat, confuses the issues. This particular point was never raised by me in the address. What I said was: "Two cardinal facts made the downfall of phlogiston complete—the discovery of oxygen, and the determination of the compound nature of water. M. Berthelot's contention

is, that not only did Lavoisier effect the overthrow, but he also discovered the facts." I, in common, I venture to assert, with every British chemist, admit unreservedly that Lavoisier effected the overthrow, but we deny that he discovered the facts. It is altogether beside the question for M. Berthelot now to say in effect: "Have I not praised your men of science, and thereby drawn down upon myself the wrath of my countrymen? And yet you are not satisfied!" We are sorry for M. Berthelot; he is in the position of the man with many friends, and his friends for the moment are a little angry. He has either not the courage of his convictions, or he has halted between two opinions —with the usual consequences.

With respect to the discovery of the compound nature of water, M. Berthelot now takes up a different position from that which he occupies in *La Révolution Chimique*. His contention there was that by every legitimate canon the experiment of 24th June 1783 gives to Lavoisier the priority of discovery. He now admits that Cavendish played "un rôle capital—car il donna le branle aux esprits vers la solution définitive." But how was this possible when Cavendish's memoir was not published until January 1784? There is really only one answer—it was given simply by the intervention of Blagden. I repeat that Blagden told Lavoisier of Cavendish's researches and of his conclusions, and that it was in the light of that knowledge that the experiment of 24th June 1783 was made. There can be no question of this. Blagden's testimony, as given in the letter to Crell, is as direct and decisive as it is damning. It was never contradicted by Lavoisier, nor by Laplace, Vandermonde, Fourcroy, Meusnier, or Legendre, who were present on the

occasion when Lavoisier himself admits that he received the information. M. Berthelot does not contradict it, but, instead, he asperses the moral character of Blagden. This method of treating a witness whose evidence cannot be rebutted is apt, when unsuccessful, to recoil on him who attempts it. It is perfectly true that Blagden interpolated the famous passage in Cavendish's memoir :—

> During the last summer, also, a friend of mine gave some account of them [the experiments] to M. Lavoisier, as well as of the conclusion drawn from them. . . . But at that time, so far was M. Lavoisier from thinking any such opinion warranted that, till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water.

This passage, however, was inserted with Cavendish's knowledge and consent, and by his assistant and amanuensis, who happened to be the very man who had a personal knowledge of the facts. Assuming the statement to be true, where is the immorality of the proceeding?

Everything that we can learn authoritatively concerning Blagden goes to show that he was an upright and honourable man. Sir Joseph Banks has testified to his abilities and integrity. Dr. Johnson spoke of his copiousness and precision of communication, with the characteristic addition : " Blagden, sir, is a delightful fellow." Laplace, Cuvier, Berthollet, and Benjamin Delessert were among his friends.[1] He was rich, and

[1] Many of the letters of Berthollet to Blagden are still in existence. In one of these, dated "19 Mars, 1785," he writes from Paris :—"L'on s'est beaucoup occupé ici ces derniers tems de la belle découverte de Mr. Cavendish, sur la composition de l'eau : Mr. Lavoisier a tâché de porter sur cet objet toute l'exactitude dont il est susceptible. . . . Mr. Lavoisier veut répéter l'expérience en faisant brûler l'air dephlogistiqué dans le gas inflammable, et il y a apparence qu'alors on n'aura point d'acide nitreux, selon les belles observations de Mr. Cavendish." Is this language consistent with the belief that Berthollet, who must have known the facts, regarded Lavoisier as the real discoverer of the compound nature of water?

was understood to have speculated to profit in the French funds. For thirteen years he was a Secretary of the Royal Society, and in 1792 he was knighted for his services to science. Every year he spent a considerable time on the Continent, and was frequently in Paris. The gossip of the period states that he aspired to the hand of Madame Lavoisier, who preferred Count Rumford. He died in Berthollet's house at Arcueil, on 26th March 1820. In an obituary notice in the *Moniteur* of 22nd September 1820, M. Jomard testifies to his benevolence and generosity, and states that "none of his countrymen have done more justice to the labours and discoveries of the French, or have contributed more than he to the happy relations which have subsisted for six years (1814-20) between the *savans* of the two countries." By his will he provided liberally for his scientific friends: Berthollet, the daughter of Madame Cuvier, and the daughter of Count Rumford each received £1000; and Laplace £100, "to purchase a ring." M. Berthelot asperses the character, not only of Blagden, but also of his countrymen, by his insinuations. Would he have us believe that Berthollet, Cuvier, and Laplace would extend their friendship to, and receive pecuniary benefits from, one whom they believed had foully stabbed their compatriot in the back? It is surely incumbent on M. Berthelot, on every ground, either to substantiate his implications or to withdraw them.

M. Berthelot makes the gratuitous assumption that I am ignorant of the work of Monge. Whether I am or not is altogether beside the mark. There is, indeed, no question of Monge. Monge distinctly disclaims priority to Cavendish, nor did he attempt to establish a right to be considered an independent discoverer of

the true nature of water. In his memoir, "Sur le Résultat de l'Inflammation du Gas inflammable et de l'Air dephlogistiqué dans les Vaisseaux Clos," he tells us that the experiments recorded in it were made in June and July 1783, and repeated in October of the same year. "I did not then know," he adds, "that Mr. Cavendish had made them several months before in England, though on a smaller scale; nor that MM. Lavoisier and Laplace had made them about the same time at Paris in an apparatus which did not admit of as much precision as the one which I employed." I fail to see what M. Berthelot gains by his reference to Monge.

M. Berthelot reproaches Priestley and Cavendish for their adherence to phlogistonism. I say it with all respect—but is it seemly for M. Berthelot, of all men, to cast this stone? Is not he himself an exemplification of that conservatism which he deplores? A generation ago the doctrine of Avogadro became the corner-stone of that edifice of which M. Berthelot asserts that Lavoisier laid the foundations. Indeed, the introduction of that doctrine effected a revolution hardly less momentous than that of which Lavoisier was the leader. But what has been M. Berthelot's consistent attitude towards this teaching? We can illustrate it by a single example. He is the sole teacher in Europe of any position who continues to symbolise the constitution of that very substance of which he claims that Lavoisier discovered the composition by a formula which is as obsolete as any conception of phlogistonism.

VIII

MICHAEL FARADAY

A Review of Dr. Bence Jones's "Life and Letters of Faraday."
—*Manchester Guardian*, 1870

Michael Faraday, one of the greatest experimental philosophers of this or indeed of any other century, was born at Newington, in Surrey, on the 22nd September 1791. Shortly after the birth of Michael, their third child, his parents settled permanently in London; but through the continued ill-health of the father, who was a blacksmith by trade, the family were always in straitened circumstances. During the distress of 1801 they received public relief, and to the little Michael one loaf of bread was given each week, and it had to serve him for that length of time. Of his mother Faraday always spoke in the most affectionate terms, and to her care and solicitude may be attributed the great influence which his home had upon his character. Although unable to enter into his occupations, she was exceedingly proud of her son; so much so that Faraday asked his wife not to talk to his mother so much about him or his honours, saying she was quite proud enough of him, and it would not be good for her. Poor as the parents were, they managed to afford their children some little school learning, and Michael obtained the rudiments of reading, writing, and arithmetic at a common day-

school. When twelve years old the young Faraday went on trial for a year as an errand-boy to Mr. George Riebau, a bookseller and bookbinder in Blandford Street, near Manchester Square. He tells us "that it was his duty, when he first went, to carry round the papers that were lent out by his master. Often on a Sunday morning he got up very early and took them round, and then he had to call for them again; and frequently, when he was told the paper was not done with, 'You must call again,' he would beg to be allowed to have it, for his next place might be a mile off, and then he would have to return over the ground again, losing much time and being very unhappy if he was unable to get home to make himself neat, and to go with his parents to their place of worship." The Faradays were members of a Sandemanian congregation, and to this sect Faraday adhered throughout his life, and for many years was an elder of their chapel.

We are told that in after-life the remembrance of his earliest occupation was often brought to the mind of Faraday. One of his nieces said that he rarely saw a newspaper-boy without making some kind remark about him. "I always feel a tenderness for those boys," he said on one occasion, "because I once carried newspapers myself."

The year of probation having expired, Faraday was apprenticed to his master, who, as it is written in the indentures, required no premium in consideration of his previous faithful service. The young Faraday was by no means precocious in disposition, but his great originality of mind quickly showed itself. Few books passed under his hands without his obtaining some knowledge of their contents. Watts *On the Mind*, he said, first made him think, and Mrs. Marcet's *Conversa-*

tions on Chemistry, and the article "Electricity," in an encyclopædia he was employed to bind, first turned his attention to science. His amiability, and the intelligent nature of his conversation, soon attracted the attention of his master's customers; and one of them, a Mr. Dance, afforded him the opportunity of hearing Sir Humphry Davy lecture at the Royal Institution. Davy was then near the zenith of his fame; his brilliant discovery of the compound nature of the alkalis and alkaline earths created an epoch in the history of chemistry, and stamped the discoverer as one of the greatest workers of his time. Probably no English philosopher ever enjoyed a higher degree of popularity than did Davy about this time. Science was just then in fashion, and chemistry all the rage. Albemarle Street was often blocked with the carriages of the fine ladies and gentlemen who thronged to hear the great chemist expound the new theories which his own discoveries had helped to inaugurate. Some may think that there was much in the career of Davy to have excited the young Faraday's aspirations; in their origin at least the baronet and the bookbinder had something in common, and by the same steps by which the one had risen the other might mount. But it is more than probable that then, and for a long time subsequently, the desire to emulate the titled lecturer never once crossed the mind of his humble auditor, perched over the clock in the gallery. Davy's example may have stimulated his hopes, but it did not create them. The bias to his inclinations had already been given. Faraday's occupation as a bookbinder was going, if not gone. His longing to escape from trade continually breaks out in his correspondence. "The desire," he says, "to be engaged in scientific occupation,

even though of the lowest kind, induced me to write, in my ignorance of the world and simplicity of my mind, to Sir Joseph Banks, then President of the Royal Society. Naturally enough 'no answer' was the reply left with the porter." His thoughts at this time, when he was "giving up trade and taking to science," are well seen in his letters to his friend Abbott. Mr. Abbott was a confidential clerk in the City, whose acquaintance Faraday had made during a course of lectures on natural philosophy given by Mr. Tatum. Abbott was apparently well educated, and the characteristic deference paid by Faraday to his friend's superior school-learning may repeatedly be seen in the course of their long correspondence. "These letters to Abbott," says Dr. Bence Jones, "possess an interest almost beyond any other letters which Faraday afterwards wrote. It is difficult to believe that they were written by one who had been a newspaper-boy, and who was still a bookbinder's apprentice, not yet twenty-one years of age, and whose only education had been the rudiments of reading, writing, and arithmetic. Had they been written by a highly educated gentleman, they would have been remarkable for the energy, correctness, and fluency of their style, and for the courtesy, kindness, candour, deference, and even humility of the thoughts they contain." This is high praise, but it is fully merited. Some of the letters are indeed models of epistolary style.

Faraday was not content with simply listening to Davy, but made careful notes of the lectures, afterwards writing them out in a fuller form, and interspersing them with such drawings as he could make. His own words to Dr. Paris, the biographer of Davy, written seventeen years after, will best show what he then did

with these notes: "My desire to escape from trade, which I thought vicious and selfish, and to enter into the service of science, which I imagined made its pursuers amiable and liberal, induced me at last to take the bold and simple step of writing to Sir Humphry Davy, expressing my wishes, and a hope that if an opportunity came in his way he would favour my views; at the same time I sent the notes I had taken of his lectures." Davy, at their first meeting, wished to dissuade the young aspirant from the course which he contemplated taking, telling him that science was a harsh mistress, and in a pecuniary point of view but poorly rewarded those who devoted themselves to her service. He smiled at Faraday's notion of the superior moral feelings possessed by scientific men, and said that a few years' experience would set him right on this matter;—which it most certainly did, as we shall hereafter show. Some time after this interview Faraday was startled late at night by a loud knock at the street door, and was still more astonished to see a footman alight from a carriage and leave a note for him. The note was from Sir Humphry to inform him that the position of laboratory-assistant at the Royal Institution was then vacant, and if he were still minded to leave his present occupation, the place was at his disposal. A few days after Faraday commenced his duties at Albemarle Street at a salary of 25s. a week, with two rooms at the top of the house.

Such was the origin of his connection with the Royal Institution—a connection which lasted nearly to the end of his long life, and which raised the Institution to the eminence it at present occupies. The very nature of his first duties there shows that he must have already acquired no inconsiderable amount of manipulative skill

from the experiments which he made in the course of his self-tuition in Blandford Street. Davy was at that time engaged on the study of the so-called nitrogen trichloride, perhaps the most explosive compound known to chemists. That he should allow Faraday to assist him in a research where a single blunder might be attended with the most serious consequences, indicates that Davy, himself no mean experimenter, fully recognised the other's ability. Throughout the course of the investigation Faraday was exposed to constant peril, and it was only through continual care and precaution that no very sad results attended the numerous explosions of this singularly unstable compound.

In the autumn of this year (1813) Sir Humphry Davy proposed to go abroad, and offered to take Faraday with him as amanuensis, promising that he should resume his situation at the Institution upon his return. During his travels on the Continent with Davy, which lasted about a year and a half, Faraday kept a journal, in which he noted down his impressions of the journey. This journal, which occupies a considerable portion of the first volume of Dr. Bence Jones's work, is as remarkable for the vividness and accuracy of the descriptions of what he saw, as for the entire absence of any particulars relating to those with whom he travelled. His cautious reticence on this latter point is scarcely less apparent in his letters; but the little that does escape him shows that his journey was not one of unmixed pleasure. His relation to Sir Humphry Davy was not very clearly defined; to his duties as an amanuensis he was not unfrequently obliged to add those of a valet, and this service was especially annoying to him, the more so that it was entirely unexpected. Perhaps no one ever possessed more real humility than did Faraday, and the

very genuineness of his humility is shown by its never degenerating into servility. In one of his letters to Abbott he writes: "I fancy I have cause to grumble, and yet I can scarcely tell why. If I approve of the system of etiquette and valuation formed by the world, I can make a thousand complaints; but, perhaps, if I acted influenced by the pure and unsullied dictates of common sense, I should have nothing to complain of, and therefore all I can do is to give you the circumstances." He then relates how it came to pass that he was obliged to undertake duties so irksome to him. But he concludes by saying that it was, perhaps, the name more than the thing which hurts. "I should have but little to complain of were I travelling with Sir Humphry alone, or were Lady Davy like him; but her temper makes it oftentimes go wrong with me, with herself, and with Sir Humphry." In another letter he writes: "I am quite ashamed of dwelling so often on my own affairs, but as I know you wish it, I shall briefly inform you of my situation.". . . He then goes on to say that Sir Humphry was unable to meet with a valet to his satisfaction. "This, of course, throws things into my duty which it was not my agreement, and is not my wish, to perform, but which are, if I remain with Sir Humphry, unavoidable. These, it is true, are very few; for, having been accustomed in early years to do for himself, he continues to do so at present, and he leaves very little for a valet to perform; and as he knows that it is not pleasing to me, and that I do not consider myself as obliged to do them, he is always as careful as possible to keep those things from me which he knows would be disagreeable. But Lady Davy is of another humour. She likes to show her authority, and at first I found her extremely earnest in mortifying me.

This occasioned quarrels between us, at each of which I gained ground and she lost it; for the frequency made me care nothing about them, and weakened her authority, and after each she behaved in a milder manner."

But his ardent desire for improvement and the opportunity he possessed of adding to his knowledge of chemistry and of the other sciences continually induced him to proceed. At Geneva Faraday met Volta, but the only record of his interview with the great electrician is contained in the following lines:—"Friday 17th. Saw M. Volta, who came to Sir Humphry Davy, a hale, elderly man, bearing the red ribbon, and very free in conversation." If the young and diffident amanuensis could have foreseen the glorious destiny that awaited him, what emotions this meeting must have raised. At Geneva Faraday also made the acquaintance of De La Rive, who, "undazzled by the brilliancy of Davy's reputation, was able to see the true worth of his assistant." This led him on one occasion to place Faraday even then on a par with Davy. He invited them both to dinner. Davy, it is said, declined to dine with one who, in some things, acted as his servant. De La Rive expressed his feelings only by saying that it would then be necessary for him to give two dinners instead of one. In 1858 Faraday wrote to Mr. A. De La Rive: "I have some such thoughts (of gratitude) even as regards your own father, who was, I may say, the first who personally at Geneva, and afterwards by correspondence, encouraged, and by that sustained me."

Having thus travelled through France, Italy, Switzerland, and the Tyrol, Faraday returned to England in the spring of 1815, and immediately resumed his duty at the Royal Institution. Davy could no longer withhold

the acknowledgment of the energy and worth of his assistant. On the other hand, Faraday had now full knowledge of "his master's genius and power. He had compared him with the French philosophers whilst helping him in his discovery of iodine; and he was just about to see him engage in those researches on fire-damp and flame which ended in the glorious invention of the Davy Lamp, and gave to Davy a popular reputation even beyond that which he had gained in science by the greatest of all his discoveries—potassium." Dr. Jones is slightly in error here; iodine was discovered in 1812 by Courtois, a saltpetre manufacturer in Paris; Sir Humphry Davy, however, first conclusively demonstrated its elementary nature.

In the following year Faraday commenced lecturing. He delivered his first lecture on January 17, 1816, at the City Philosophical Society, on "the general properties of matter." In the very full notes which Dr. Bence Jones has preserved to us of this and of the subsequent lectures delivered during the year at the same Society, we think we can trace the germs of that great success which attended Faraday's career as a lecturer. These notes are remarkable for their originality of thought and fulness of illustration. In his accurate digests of contemporary knowledge we have evidence of the diligence with which Faraday set about educating himself for his new vocation. Lecturing became with him an art, to be carefully and systematically studied. His letters to Abbott, and his commonplace book, show that he had previously framed pretty accurate ideas respecting the province and functions of the lecturer whilst attending Mr. Brande and Mr. Powell in their lectures at the Royal Institution. He now took private lessons in elocution, and prevailed

on his teacher to attend his lectures in order that the errors of his address and delivery might be brought home to him. Eleven years after the date of this early attempt, Faraday delivered his first lecture at the Royal Institution. As he steps upon the place where, fifteen years before, Davy had stood, we wonder if his thoughts revert to the memory of the young bookbinder's apprentice, seated in the gallery over the clock! "For thirty-eight years," says his biographer, "his lectures were the life of the Royal Institution. His singular power of making himself one of his audience was felt in his juvenile lectures, in his theatre courses, and in his Friday evening addresses. In his juvenile lectures, his simple words and his beautiful experiments, his quickness and his clearness, kept the attention and fixed his instruction in the mind even of the youngest of his hearers, whilst the most practised teacher would find old experiments shown in a new form, which the genius of Faraday only could have invented, and which his handicraft enabled him to carry out. In his theatre lectures his matter was always over-abundant, his experiments were always successful, his knowledge was always at the furthest limits to which it had at the time been extended by himself or by others, and yet his consideration for those who knew but little never failed. But it was in his Friday evening discourses that his great power as a lecturer came out. His manner was so natural that the thought of any art in his lecturing never occurred to any one. Rapidly, and yet clearly, he made the object of his lecture known. Those who had but little knowledge could see his starting-point, and they thought they saw where he was going. Those who knew most followed him beyond the bounds of their own knowledge, forgetting almost the lecturer, who

seemed to forget himself, in his words and his experiments, and who appeared to be trying only to enable them to judge what his latest discoveries were worth; and when he brought the discoveries of others before his hearers, one object, and one alone, seemed to determine all he said and did, and that was, 'without commendation and without censure, to do the utmost that could be done for the discoverer.'"

In 1821, when twenty-nine years of age, Faraday married Miss Sarah Barnard, the daughter of an elder of the Sandemanian Chapel. Many years after he wrote in the notes of his own life: "On June 12, 1821, he married — an event which more than any other contributed to his earthly happiness and healthful state of mind. The union has continued for twenty-eight years, and has nowise changed, except in the depth and strength of its character."

About this time Faraday received his first scientific honour; it came from the Cambridge Philosophical Society. Perhaps no scientific man ever obtained during his lifetime such an extended recognition of his services as did Faraday; in all he received no less than ninety-five honorary titles and marks of merit from the various learned societies scattered throughout Europe and America. In 1838 he wrote: "One title, that of F.R.S., was sought and paid for; all the rest are spontaneous offerings of kindness and goodwill." Writing, in 1854, to Lord Wrottesley, then Chairman of a Parliamentary Committee of the British Association, he says: "Through the kindness of all, from my Sovereign downwards, I have that which supplies all my need; and in respect of honours I have, as a scientific man, received from foreign countries and sovereigns those which, belonging to very limited and

select classes, surpass, in my opinion, anything that it is in the power of my own to bestow. I cannot say that I have not valued such distinctions; on the contrary, I esteem them very highly, but I do not think that I have ever worked for or sought after them."

In 1829 he was made a member of the Scientific Advising Committee of the Admiralty, and from about this time dates his connection with the Government, which lasted nearly to the end of his life. To Lord Auckland he wrote: "I have always, as a good subject, held myself ready to assist the Government if in my power—not for pay, for except in one instance (and then only for the sake of the person joined with me), I refused to take it." The exception to which he here refers was in the case of the Haswell Colliery explosion, when he was sent down by the Government, together with Sir (then Mr.) Charles Lyell, to attend the inquest. Faraday, however, squared the account with his conscience by subscribing most liberally to the fund raised for the widows and children of those who had perished in the catastrophe.

In 1836 he was appointed Scientific Adviser to the Trinity House; and his work, extending over a period of thirty years, was of the most miscellaneous character, including "the ventilation of lighthouses; the arrangements of their lightning conductors; the analysing and supervising of their drinking waters; the examination of their optical apparatus; the determination of the worth of the different propositions made to the Trinity House regarding the lights, extending from the practical use of the magneto-electric light down to the samples of cottons, oils, and paints that were used." When he was appointed he spoke to the deputy-master "of his indifference to his proposition as a matter of interest,

though not as a matter of kindness." The value of this appointment was, in the words of Dr. Jones, "an unlimited amount of kindness and £200 a year."

In 1816 Faraday made his first contribution to science. It consisted of an analysis of a specimen of native caustic lime. Sir Humphry Davy had given him the analysis to make as a first attempt in chemistry "at a time," as he tells us, "when his fear was greater than his confidence, and both far greater than his knowledge; at a time, also, when he had no thought of ever writing an original paper on science." His activity of mind at this time was marvellous. From the date of his first paper until 1820 he contributed no less than thirty-seven notices and papers to the *Quarterly Journal of Science.*

But the work which made Faraday the wonder of the scientific world was yet to be done, and the records of the next eleven years show how carefully and how patiently he educated himself for his great task. In 1823 he published his first paper on the condensation of the gases. The first gas which he liquefied was chlorine. Acting upon a suggestion made by Davy, Faraday, who was then working upon the hydrate of chlorine, sealed up some of the crystals of this substance in a bent tube, and subjected them to heat. The result was the formation in the bent portion of the tube of a quantity of a yellow oily liquid which, as Faraday "puzzled out" for himself, could only be condensed chlorine.

This discovery immediately attracted universal attention; but the merit of it was claimed by another. The claimant was no other than Davy. A feeling of jealousy had gradually been growing up in the mind of the Honorary Professor of Chemistry at the Royal

Institution towards his assistant, and this discovery exposed it to the world. Charges of plagiarism were freely whispered about, but it was only after the lapse of many years, when the question was again revived by the late Dr. Davy in the Life of his illustrious brother, that Faraday was prevailed upon to set the matter in its proper light. Faraday was always very much averse to scientific controversy, and it was difficult to move him to take up his pen in his own defence. Many years afterwards he attempted to mediate between Matteucci and Du Bois Raymond, who were at variance. Writing to the former, he says: "Who has not to put up in his day with insinuations and misrepresentation in the accounts of his proceedings given by others, bearing for the time the present injustice, which is often unintentional and often originates in hasty temper, and committing his fame and character to the judgment of the men of his own and future time. I see that that moves you which would move me most—namely, the imputation of a want of good faith; and I cordially sympathise with any one who is so charged unjustly. Such cases have seemed to me almost the only ones for which it is worth while entering into controversy. I have felt myself not unfrequently misunderstood, often misrepresented, sometimes passed by, as in the cases of specific inductive capacity, magneto-electric currents, definite electrolytic action, etc.; but it is only in the cases where moral turpitude has been implied that I reply. . . . These polemics of the scientific world are very unfortunate things; they form the great stain to which the beautiful edifice of scientific truth is subject. Are they inevitable? They surely cannot belong to science itself, but to something in our fallen nature." Faraday's defence of himself in the affair of

the condensation of the gases is very explicit. It was written to his friend, Richard Phillips, and was published in the *Philosophical Magazine* for 1836. He concludes it as follows :—

I have never remarked upon or denied Sir H. Davy's share of the condensation of chlorine or the other gases; on the contrary, I think that I long ago did him full justice in the papers themselves. How could it be otherwise? He saw and revised the manuscripts; through his hands they went to the Royal Society, of which he was president at the time; and he saw and revised the printer's proofs. Although he did not tell me of his expectations when he suggested heating the crystals in a closed tube, yet I have no doubt that he had them; and, though perhaps I regretted losing my subject, I was too much indebted to him for much previous kindness to think of saying that that was mine which he said was his. But observe (for my sake) that Sir H. Davy nowhere states that he told me what he expected, or contradicts the passages in the first paper of mine, which describe my course of thought, and in which I claim the development of the actual results. All this activity in the condensing of gases was simultaneous with the electro-magnetic affair, and I had learned to be cautious upon points of right and priority. When, therefore, I discovered in the course of the same year that neither I nor Sir H. Davy had the merit of first condensing the gases, and especially chlorine, I hastened to perform what I thought right, and had great pleasure in spontaneously doing justice and honour to those who deserved it. Monge and Clouet had condensed sulphurous acid probably before the year 1800; Northmore condensed chlorine in the years 1805 and 1806 (*Nicholson's Journal*). I therefore published on January 1 in the following year, 1824, a historical statement of the liquefaction of gases (*Quarterly Journal of Science*). . . . The value of this statement of mine has since been fully proved, for, upon Mr. Northmore's complaint, ten years after, with some degree of reason, that great injustice had been done to him in the affair of the condensation of gases, and his censure on the "conduct of Sir H. Davy, Mr. Faraday, and several other philosophers for withholding the name of the first discoverer," I was able, by referring to the statement, to convince him and his friends that if my papers had done him

wrong, I at least had endeavoured also to do him right (*Philosophical Magazine*, 1834). Believing that I have now said enough to preserve my own "honest fame" from any injury it might have risked from the mistakes of Dr. Davy, I willingly bring this letter to a close, and trust I shall never again have to address you on the subject.

Davy's jealousy culminated in active opposition to the election of Faraday as a Fellow of the Royal Society. His certificate as a candidate was drawn up by his friend Phillips, and the first signatures were those of Wollaston, Children, Babington, and Sir W. Herschel. Many years after Faraday gave the following account of what had passed between him and Davy relative to the matter of his election: "Sir H. Davy told me I must take down my certificate. I replied that I had not put it up; that I could not take it down, as it was put up by my proposers. He then said I must get my proposers to take it down. I answered that I knew they would not do so. Then he said: I, as president, will take it down. I replied that I was sure Sir H. Davy would do what he thought was for the good of the Royal Society." One of Faraday's proposers bears witness that "Sir H. Davy had walked for an hour round the courtyard of Somerset House arguing that Faraday ought not to be elected." Davy's prediction to the aspiring young bookbinder that his notions of the superior moral feelings of philosophic men would be set right by a few years' experience was signally verified. But on the ballot being taken, Faraday was almost unanimously elected; there was only one black ball.

About this time Faraday discovered benzene, or, as he then termed it, bicarburet of hydrogen, among the products of the condensed oil-gas manufactured by

the Portable Gas Company. Faraday may thus be said to have laid the foundation of that immense industry which recent discoveries in chemistry have so rapidly developed — the manufacture of the so-called aniline dyes. Enormous quantities of benzene are now employed in the production of these beautiful colours.

In 1831 the great work of his life commenced by the publication in that year of the first series of his immortal *Experimental Researches in Electricity*. Our limited space entirely prevents any attempt to do justice to the nature and extent of his researches on magneto-electricity, voltaic induction, definite electro-chemical decomposition; nor, we regret to add, can we refer our readers to the book before us for a more detailed account. In our opinion Dr. Bence Jones fails to present any clear conception of the nature of Faraday's discoveries. Dr. Jones has in one sense performed his duty as a biographer most conscientiously, perhaps rather too conscientiously; like the famous Cid Hamet, he is the most punctual and diligent searcher after the minutest circumstances, "even to the very atoms of his true history," and the result is that too much is left to the judgment and discrimination of his reader in the matter of Faraday's discoveries. Thus we see on the same page, with no attempt at distinction, an account of a paper on the general magnetic relations and characters of the metals, together with one on such a comparatively unimportant subject as a supposed new sulphate and oxide of mercury. Happily, however, Faraday does not want an interpreter. We can refer our readers to no clearer exponent of his labours than Professor Tyndall, whose charming book, *Faraday as a Discoverer*, is eminently worthy the attention

of those who desire to know more of the great philosopher's work.

We have already spoken of Faraday as a good subject, and we should like to dwell for a moment on the manner in which he fulfilled his duties as a good citizen. He took the warmest interest in all the great questions of the day. His desire for sanitary reform was the occasion of his letter to the *Times* on the state of the river Thames. Who is not familiar with Leech's cartoon of Faraday giving his card to Father Thames, with the hope that the "Dirty fellow will consult the learned professor"? Within recent years the question of the position of natural science in the various *curricula* of our schools and universities has attracted considerable attention. Faraday's opinion on this subject is especially valuable. "I do think," he says, "that the study of natural science is so glorious a school for the mind that, with the laws impressed on all created things by the Creator, and the wonderful unity and stability of matter and the forces of matter, there cannot be a better school for the education of the mind." When examined before the Public School Commissioners, he said: "That the natural knowledge which had been given to the world in such abundance during the last fifty years, I may say, should remain untouched, and that no sufficient attempt should be made to convey it to the young mind, growing up and obtaining its first views of these things, is to me a matter so strange that I find it difficult to understand; though I think I see the opposition breaking away, it is yet a very hard one to be overcome. That it ought to be overcome I have not the least doubt in the world."

In 1835 Faraday was told that Sir Robert Peel, then Prime Minister, contemplated offering him a pension of

£300 a year. His first impulse was to refuse it, on the ground that he could not accept a pension whilst he was able to work for his living. He was induced, however, by his relatives to reconsider his determination, and waited upon Lord Melbourne, who was then in office, to learn his intentions in the matter. During their conversation his lordship expressed himself, as he himself admits, in rather "too blunt and inconsiderate a manner." According to Dr. Jones it is probable that he designated the system of giving pensions to literary and scientific men as a piece of humbug. However, on the same evening, Faraday left his card with the following note at Lord Melbourne's office :—

October 26.

My Lord — The conversation with which your Lordship honoured me this afternoon, including, as it did, your Lordship's opinion of the general character of the pensions given of late to scientific persons, induces me respectfully to decline the favour which, I believe, your Lordship intends for me; for I feel that I could not with satisfaction to myself accept at your Lordship's hands that which, though it has the form of approbation, is of the character which your Lordship so pithily applied to it.

The matter, however, was ultimately amicably settled, and Faraday enjoyed his well-merited pension to the end of his days.

In 1858, through the kind consideration of the Prince Consort, the Queen offered him a house on Hampton Court Green, in which he passed the rest of his life. He was now nearly seventy years of age. The dreaded reaction of that intense mental strain to which his mind had been subjected had long since set in; but thanks to the watchful care and tender solicitude of his wife, the evil consequences were but very gradual in their growth. In 1841, when he was

scarcely fifty years of age, he was obliged, through loss of memory and giddiness, to discontinue his researches for a time. He then rested almost entirely for a year, spending much of his time in Switzerland; and during the four succeeding years no further experiments in electricity were made, with the exception of an inquiry into the mode of working of Sir W. Armstrong's hydro-electric machine.

His mind was now gradually breaking up, and the knowledge of his increasing infirmities compelled him to resign, one by one, his various appointments. He now seldom left Hampton Court. A friend from London asked how he was: "Just waiting," was the reply. At another time he wrote: "I bow before Him who is Lord of all, and hope to be kept waiting patiently for His time and mode of releasing me according to His divine Word, and the great and precious promises whereby His people are made partakers of the divine nature."

On August 25, 1867, he passed quietly and peacefully away, dying, with scarcely a premonitory symptom, in his chair in his study. His funeral was strictly private; and, in accordance with his wishes, a gravestone "of the most ordinary kind" in Highgate Cemetery marks the last resting-place of one of the greatest and truest of experimental philosophers, and of one of the humblest and most tender-hearted of men.

In the foregoing sketch of the life and labours of Faraday we have had occasion more than once to offer our opinion as to the manner in which his biographer, Dr. Bence Jones, has accomplished his task. The autobiographical details of the work have been arranged with great tact and discrimination, and the desire to set forth the singular beauty and purity of his friend's

character is evident on every page. It is impossible to over-estimate the amount of good which such a book may do. On reading it one feels the more constrained to admit that "a mind fraught with integrity is the noblest possession."

IX

THOMAS GRAHAM

A Lecture (with Additions), delivered in the Yorkshire College, Leeds, introductory to the Evening Class Session, 1877-78.

Thomas Graham, one of the most original chemical thinkers of the nineteenth century, was born in Glasgow on 21st December 1805. The house in St. Andrew's Square, in the east end of the city, in which he first saw the light, is still standing. The Grahams belonged to Perthshire, but the father of the chemist, James Graham, had removed to Glasgow when very young, and ultimately became what was then styled a manufacturer. His business was sufficiently prosperous to enable him to give his son the benefit of a sound education. When scarcely six years old young Graham was sent to an English preparatory school, whence he was removed in 1814 to the High School, and at fourteen years of age he began his university career, entering, amongst others, the classes of Thomas Thomson on Chemistry and Meikleham on Natural Philosophy. He was a quiet, studious boy, conscientious and diligent in his work, but not otherwise remarkable among his fellows.

Thomas Thomson seems to have possessed, in a high degree, the faculty of communicating his own spirit of inquiry to his pupils. "Don't you think, Doctor," said

Graham on one occasion to his teacher, "that when liquids absorb gases the gases themselves become liquids?" This remark, coming from so young a pupil, and uttered at a time when the mutual relations of the physical states of matter were more vaguely understood than now, naturally impressed Thomson. The interest thus awakened in the young enthusiast never subsequently slumbered, and it was remarked by many that, in their meetings as members of the Philosophical Society of Glasgow, Thomson invariably treated Graham with an amount of respect and even deference which that brusque and quick-tempered old philosopher too frequently failed to extend to others.

Thomson's teaching gave a fixity to the purpose of Graham's after-life, and a desire to unravel some of those secrets of nature which Thomson's lectures had set him pondering upon took complete possession of his mind. His father had intended that Graham should enter the ministry, but his dislike to a calling for which he felt that he was not naturally fitted strengthened even to repugnance as he learned by a somewhat painful experience how inflexible was the determination of his parent. But Graham's mother sympathised with, even if she could not share in, the thoughts and aspirations of her son; and mainly by her self-sacrifice he was enabled to continue his studies for about two years in Edinburgh. He was in the habit of writing at great length to her, and his letters show the depth of his gratitude and affection. Whilst in Edinburgh he earned his first fee—some five pounds, by literary work—the whole of which he expended in presents to his benefactress.

In Edinburgh he attended the lectures and enjoyed the friendship of Hope and Leslie, and doubtless the

teaching of these distinguished men in no small degree contributed to direct his attention to that particular domain in science—the great borderland between physics and chemistry—in which his greatest triumphs were won. Returning to Glasgow, and acting on the advice of Meikleham, he sought to give lessons in mathematics; but he quickly threw up this precarious means of livelihood, and established a private laboratory of the most modest description in Portland Street. In 1829 he was appointed Lecturer on Chemistry at the Mechanics' Institution in place of Dr. Clark, who afterwards occupied the chemical chair at Aberdeen; but in the following year the Lectureship in Chemistry in Anderson's College fell vacant, and Graham was elected in succession to Dr. Ure, of dictionary fame. This event was the turning-point in his career. Although there was no endowment attached to the chair, and the laboratory was but scantily furnished, he had what he so long coveted—the means and the opportunity to carry out the promptings of his genius for investigation.

It would be impossible to over-estimate the influence on science of the seven years which Graham spent at Anderson's College. During that time were sown all the seeds of the rich harvest of his after-years. Whilst in Glasgow he was elected into the Royal Society of London, in whose *Transactions* he had published his memorable "Researches on the Arsenates, Phosphates, and Modifications of Phosphoric Acid." In 1837 he removed to London, as the successor of Edward Turner in the recently founded University of London, now called University College. His election was in no small degree due to the good offices of Lord Brougham, whose vote was largely determined by the encomiums which Humboldt had passed upon the young philosopher. In

Gower Street Graham's ability found adequate scope, and no teacher of chemistry in England at the time exercised a greater influence. The success of his lectures was due more to his philosophic method of exposition, to the logical arrangement of his matter, to his suggestiveness, and to the accuracy and extent of his learning, than to eloquence or fluency of speech. Not that Graham was altogether careless of such matters; he was too conscientious and painstaking a teacher to neglect any legitimate means of impressing the minds of his pupils with the ideas which he wished to convey. But even to the last he never quite overcame a certain nervousness and hesitancy of manner, which, to an occasional listener, seemed to obscure the real enthusiasm which was moving him, and of which his students were fully conscious.

In 1841 was founded the Chemical Society of London, and chemical science in England will for ever remain indebted to Graham for the share he took in the establishment of that learned body. He was its first president, and the early proceedings of the Society indicate the zeal with which he discharged the duties of his office. This interest in the diffusion of chemical knowledge was seen also in the share which he took, in 1846, in founding the Cavendish Society, which, by means of its translations, has enriched the chemical literature of this country with a series of works of great value.

By his appointment, in 1854, to the Mastership of the Mint, came what the world regarded as his crowning distinction. This position, rendered honourable by a succession of some of the most illustrious names in our scientific history, was, however, no sinecure to Graham. At this time, when it was generally thought that he

was enjoying well-merited repose, his energies were wholly devoted to the self-created duties of his office, and he was experiencing some of the most vexatious troubles of his life, since the greatest firmness was necessary to carry out the rigid system of control which his sense of rectitude prompted him to institute. Several years elapsed before his name reappeared in the *Transactions* of the Royal Society; these were years of incalculable loss to science. It was only about the year 1860 that he seems to have acquired the requisite leisure to continue his scientific work, and now memoir after memoir appeared to testify to his unwearied energy. His last paper, on Hydrogenium, was published only a few months before his death. This incessant mental activity rapidly told on a constitution naturally feeble. In the summer of 1869 he showed symptoms of decay, but his nearest friends were hardly prepared for the suddenness of his end. He died in London, at his house, No. 4 Gordon Square, on the 16th September 1869.

Graham's first paper, published when he was twenty-one years of age, in the *Annals of Philosophy* for 1826, relates to the question on which he had already speculated when a student with Thomson—that is, the absorption of gases by liquids. The supposition that gases are only vapours remote from their points of liquefaction was not unheard of even during the first years of the nineteenth century, but the fact was first definitely established in 1823, when Faraday succeeded in liquefying a considerable number of gases. Graham, reasoning from the relations of mixed liquids to heat, seeks to prove that gases are absorbed by liquids by reason of their capability of being liquefied; and he contends that solutions of gases in liquids are mixtures of a more

volatile with a less volatile liquid, and therefore obey the laws which hold for such mixtures. The detention of the gas in the solvent is owing to the mutual affinity between liquids. This affinity, which occasions the miscibility of liquids, affects the bulk or density of the mixture, and frequently diminishes the volatility of the more easily vaporised liquid in the mixture. In this way the phenomena of the absorption of gases are brought into the same class as those of the miscibility of liquids. It is unnecessary to point out in detail the objections which may be urged against this theory of gaseous-absorption. The paper is now only of historical interest. It augured well, however, for the future of the young philosopher, as it is characterised by a certain dignity of language, by precision of statement, close reasoning, and ingenuity and skill in the presentation of the argument. It was followed in the same year, first, by a short speculative paper "On the Heat of Friction," in which the attempt is made to reconcile the substantial existence of heat with its appearance in friction; and secondly, by a note on the nature of panary fermentation, which was shown to be attended with the production of alcohol. In order to avoid the possible introduction of the alcohol by the yeast or brewers' barm then exclusively employed in bread-making, Graham used leaven, and obtained from the loaf, during the process of baking, alcohol of sufficient strength to burn, and to ignite gunpowder by its combustion. Some years ago, a Mr. Hicks patented a method for recovering the alcohol volatilised in panification, but after about £20,000 had been spent in futile efforts to combat the prejudice of the London public for bread "with the gin in it," the process was abandoned.

In the early part of the following year, 1827, Graham

published in the *Philosophical Magazine* a short paper "On the Finite Extent of the Atmosphere." According to Wollaston there is necessarily a limit to the atmosphere, for the reason that the mere weight of the matter of gaseous substances must afford, at a certain degree of rarefaction, a balancing resistance to further expansion. Faraday attempted to gain evidence in support of this hypothesis from experiments on the vapour of mercury. Graham seeks to show that this limit must be reached in consequence of the decrease of temperature as we ascend through the atmosphere, whereby the air is eventually reduced to a temperature which involves the loss of its elastic state, and possibly even effects its solidification. Reasoning from analogy, he infers that the condensation of the elastic air into solid particles may be attended with emission of the accumulated stores of light and heat. "These luminous appearances will be more frequent and striking at the polar regions, from the temperature there approaching more closely to the condensing point of the gaseous substances constituting the atmosphere. Their proper sites will be the thermal poles, or points on the earth's surface of lowest temperature. . . . Let us suppose a determination to condensation to take place in the superior regions of the atmosphere at the thermal pole, the surrounding elastic air would rush in and expand, to fill the vacuity occasioned by the condensation. But this rarefaction, with its attendant fall in temperature, would frequently be productive of condensation and deposition in these masses of air themselves. In this way the tendency to condensation, originating perhaps at the thermal pole, would be widely and rapidly propagated, and the attending streams of light would appear to shoot from that point. Here we

recognise the brilliant phenomena of the aurora borealis."

A paper "On Nitrification," which appeared in the *Philosophical Magazine* for the same year, consists mainly of a critical examination of the theory of Longchamp relative to the formation of nitric acid from the elements of the atmosphere. This question, which excited the attention of chemists even before the time of Lavoisier, has only recently received an adequate solution. The only original feature in Graham's communication is in his view of the part played by carbonic acid in the process.

A communication "On Exceptions to the Law that Salts are more Soluble in Hot than in Cold Water, with a new instance," also published in the *Philosophical Magazine* for 1827, deals principally with the case of magnesium phosphate, a saturated solution of which, on heating, was found by Graham to become turbid and to deposit the salt in the anhydrous form. This phenomenon is precisely analogous to that observed by Dalton in the case of lime, and by Gay Lussac in that of sodium sulphate. The explanation given by Graham is that in all these cases the apparent anomaly is due to a change of hydration.

In the course of an inquiry made in 1827 as to the best mode of dehydrating ordinary alcohol, which Graham found to be by means of quicklime, he was led to the observation that calcium chloride, one of the substances most commonly employed by chemists as a dehydrant, forms a definite compound with absolute alcohol. Combinations of a similar nature were also observed to be produced with the nitrates of calcium and magnesium, and with the chlorides of manganese and zinc. These *alcoates*, as they are termed by Graham, are solid

crystalline substances in which the alcohol appears to play the part of water of crystallisation in the hydrated salts.

In this connection may be mentioned his paper, published long subsequently in the *Journal of the Chemical Society* (vol. iii., 1851), on "Etherification," in which he seeks to combat the generally accepted opinion that the formation of sulphovinic acid is a necessary stage in the transformation of common alcohol into ether. According to Graham the sulphuric acid acts on the alcohol in a manner similar to that in which it acts upon certain essential oils—that is, by an action akin to polymerisation. Oil of turpentine, for example, when mixed with one-twentieth of its volume of sulphuric acid, is largely converted into terebene and colophene. This view of etherification is, in fact, an expression of the contact-theory of the process advocated by Mitscherlich. Graham's experiments are suggestive, but they are hardly conclusive. At the same time, no one can read his accounts of them, or follow the argument which he bases upon them, without admitting that the last word on the theory of etherification has not yet been said.

It has long been known that a piece of phosphorus glowing in air has its luminosity instantly destroyed when immersed in oxygen. Pure oxygen at atmospheric tension has no action upon phosphorus. When the tension of the oxygen is reduced, the glow reappears, as observed by Bellani de Monza, with the simultaneous appearance of a white fume due to the formation of phosphoric oxide. If, instead of reducing its tension, the oxygen is mixed with hydrogen, nitrogen, nitric oxide, carbonic oxide, carbonic acid, etc., the glow equally reappears. Certain gases and vapours, *e.g.*

olefiant gas, naphtha, oil of turpentine, and ether, prevent the glow, whether in air or in attenuated oxygen, and no oxidation of the phosphorus is found to occur. Phosphorus may be melted and kept for any length of time at 100° without alteration in air mixed with its own volume of olefiant gas. Graham, in a paper contributed to the *Quarterly Journal of Science* in 1829, gives an account of a number of observations on the glow of phosphorus dealing with these and similar facts. He found that the proportion of the gases necessary to prevent the slow combustion depends upon the density. One four-hundredth part of olefiant gas prevents the glow in air at ordinary pressures, but the phosphorus becomes luminous even when the air is mixed with its own volume of olefiant gas if the pressure is diminished to that of half an inch of mercury. Graham offers no explanation of these facts, which are in all probability connected with the formation or presence of ozone, the existence of which was only indicated by Schönbein some years after the date of Graham's paper.

The remarkable differences in inflammability observed in phosphuretted hydrogen, depending on its origin or on the admixture of various substances with it, were long misunderstood by chemists. Heinrich Rose was disposed to believe in the existence of two isomeric modifications of hydrogen phosphide, only one of which was spontaneously inflammable. Graham, who in 1835 held "the general doctrine of Isomerism as problematical," made a number of observations on phosphuretted hydrogen, from which he concluded that its spontaneous inflammability was due to adventitious matter. He found that the addition of a minute quantity of the higher oxides of nitrogen is capable of

rendering phosphuretted hydrogen spontaneously inflammable, and he concluded that the "adventitious matter" in the ordinary spontaneously inflammable variety was a volatile compound of phosphorus and oxygen, analogous to nitrous acid. Subsequent observations have shown that Graham was right in the surmise that the spontaneous inflammability was due to "adventitious matter," but wrong as to its nature. The effect is caused, as shown by Thénard, by the presence of the dihydride of phosphorus.

Graham's first important experimental work relates to the absorption of vapours by liquids. An account of the investigation was given to the Royal Society of Edinburgh on 3rd March 1828, and is printed in the *Edinburgh Journal of Science* for the same year. He shows that a relation exists between the boiling-point of a saline or acid solution and its power of absorbing the vapour of the solvent. The higher the boiling-point of the solution the greater the amount of the vapour absorbed. The degree of absorption is apparently independent of the nature of the salt. A saturated solution of common salt, which contains less than a third of its weight of non-deliquescent saline matter, absorbs aqueous vapour much more powerfully than a solution of the deliquescent carbonate of potash in thrice its weight of water. Even sea-water is capable of absorbing moisture from air saturated with it at the same temperature. A number of saline and acid solutions made of such strength that they all boiled at the same temperature, viz. 224° Fahr., were found to absorb practically the same amount of aqueous vapour. A solution of hydrochloric acid exposed to damp air absorbs moisture until its specific gravity is diminished to 1·096; on the other hand, an acid

solution of lower specific gravity evolves aqueous vapour until its specific gravity becomes 1·096, when it acquires a constant boiling-point, as already observed by Dalton.

In a short paper "On the Influence of the Air in determining the Crystallisation of Saline Solutions," originally published in the *Philosophical Magazine* for 1828, Graham deals with a question which had already attracted considerable attention long before his time, but which, in spite of much subsequent work, is still imperfectly understood, viz. the causes which induce the crystallisation of supersaturated solutions. By confining hot saturated solutions of Glauber's salt in phials or other vessels over warm mercury, Graham showed that solidification could be induced in the liquid when cold by the admission of certain gases, which, by their solution, cause the precipitation of a certain amount of the salt, and so bring about crystallisation of the excess of saline matter. According to Graham the influence of the gas is related to the degree in which it is absorbed by the solvent: the more soluble the gas the more readily does it effect crystallisation.

In the following year (1829) Graham published his first paper on the subject on which his fame chiefly rests. It is entitled "A Short Account of Experimental Researches on the Diffusion of Gases through each other, and their separation by Mechanical Means," and appeared in the *Quarterly Journal of Science*, ii., 1829, pp. 74-83. The experimental work was of the simplest possible description. A cylindrical vessel, shaped like a test-tube, about 9 inches in length and 0·9 inch internal diameter, was divided into 150 equal parts, and provided with an accurately ground glass

stopper, through which was fitted, also by grinding, a short piece of stout tube 0·12 inch in bore and 2 inches in length, and bent in the middle at right angles. The vessel was filled in succession with various gases, the stopper and tube inserted, and the whole supported in a horizontal position upon a frame, with the end of the bent tube pointing upwards when the contained gas was heavier than air, and downwards when the gas was lighter, to avoid any tendency of the gas to *flow* out of the receiver. After the gas had been allowed to diffuse into the air through the tube for a certain time, the cylindrical vessel was transferred to the pneumatic trough, and the quantity of air which had entered, and of gas that remained, ascertained. It was found that as much hydrogen gas left the vessel in two hours as of carbonic acid in ten hours. Hence hydrogen is five times more diffusive than carbonic acid. Graham concludes: "It is evident that the diffusiveness of the gases is inversely as some function of their density—apparently the square root of their density." He next studied the diffusion of a mixture of gases into air in order to determine whether each gas left the vessel independently of the other, and in the ratio of its individual diffusiveness, and he found that "in the case of mixed gases the law is—that the more diffusive gas leaves the receiver in a *greater* proportion than in the case of the solitary diffusion of the same gas, and the less diffusive gas in the mixture in a *less* proportion than in its solitary diffusion."

In a subsequent paper, entitled "On the Law of the Diffusion of Gases," read before the Royal Society of Edinburgh, 19th December 1831, and published in the *Philosophical Magazine*, ii., 1833, Graham definitely establishes the relation between the rate of diffusion of

a gas and its density, and states this relation in these words : " The diffusion or spontaneous intermixture of two gases in contact is effected by an interchange in position of indefinitely minute volumes of the gases, which volumes are not necessarily of equal magnitude, being, in the case of each gas, inversely proportional to the square root of the density of that gas." He refers in the outset to the observation of Döbereiner on the escape of hydrogen through a fissure or crack in a glass bell-jar filled with the gas and standing over water in the pneumatic trough, when it was found that the water gradually rose in the jar to a height of several inches above the level of that in the trough. With oxygen, nitrogen, or atmospheric air, no such phenomenon was observed. Döbereiner was disposed to regard the effect as due to the capillary action of the fissure; he supposed that the hydrogen only is attracted by the fissure, and escapes through it by reason of the extreme smallness of its atoms. Graham points out the insufficiency of this explanation. As already shown by Saussure, hydrogen, of all gases, is absorbed or condensed in smallest quantity by charcoal and other porous substances; and, moreover, there is no *a priori* reason to suppose that the particles of hydrogen are smaller than those of other gases. On repeating the experiment he found that, as the hydrogen escaped, a certain amount of air invariably entered the jar, through the fissure, in its place; and if care was taken to maintain the water in the jar at the level of that in the pneumatic trough, so that the air should not be drawn in nor the hydrogen thrust out mechanically, then the air which entered by diffusion amounted to between one-fifth and one-fourth of the volume of the hydrogen which passed out of the bell-

jar in the same time. Similar experiments were made with other gases, but the method was not sufficiently accurate to furnish precise results, although these were at all times compatible with, and indeed illustrative of, the law. Graham next tried tubes made of unglazed stoneware, such as was used by Priestley and other chemists at the latter end of the eighteenth century for furnace experiments, and found that the effects were much more striking. Indeed Priestley himself had discovered that the gases of the fire would readily permeate such vessels. From unglazed stoneware he passed to plaster of Paris or stucco, and this material was found so suitable that the rest of the experiments detailed in the memoir were made by means of it. The apparatus employed was extremely simple. It consisted of a glass tube from 6 to 14 inches in length and half an inch in width, fitted at one end with a thin stucco plug, about a fifth of an inch in thickness. The diffusion tube, as the instrument was termed, was graduated into hundredths of a cubic inch. It was filled with the particular gas under investigation over the mercurial or water pneumatic trough, with special precautions in the latter case to avoid wetting the plug. The tube over the trough was then allowed to stand in free contact with the air for a certain time, after which the ratio of the volume of the gas remaining in the tube to that of the air which had entered through the stucco was determined by analysis. Thus in the case of hydrogen, if we take the specific gravity of this gas as 0·0694 (air = 1), the square root of this number is 0·2635; and according to the law of diffusion 1 volume of hydrogen should be replaced by 0·2635 volume of air—that is, 1 of air should replace 3·7947 of hydrogen. The mean of a large number of

experiments, made with instruments varying in shape and size, showed that the amount of hydrogen replacing 1 volume of air was 3·83 volumes, in fair accord with the theoretical number. The following table contains the results of experiments made with such of the gases as were available at the time :—

TABLE OF EQUIVALENT DIFFUSION-VOLUMES OF GASES.
AIR = 1.

	By Experiment.	By Theory.	Specific Gravity.
Hydrogen	3·83	3·7947	0·0694
Carburetted Hydrogen . .	1·344	1·3414	0·555
Olefiant Gas	1·0191	1·0140	0·972
Carbonic Oxide . . .	1·0149	1·0140	0·972
Nitrogen	1·0143	1·0140	0·972
Oxygen	0·9487	0·9487	0·111
Sulphuretted Hydrogen . .	0·95	0·9204	1·1805
Protoxide of Nitrogen . .	0·82	0·8091	1·527
Carbonic Acid . . .	0·812	0·8091	1·527
Sulphurous Acid . . .	0·68	0·6708	2·222

The agreement between theory and experiment, although not absolute, is sufficiently close to leave no doubt of the validity of the law. Perfect concordance, indeed, could not be expected. All the gases are condensed to a greater or less extent by the porous stucco ; moreover, the water of hydration of the plaster affects the results in some instances. Indeed in the cases of chlorine, hydrochloric acid gas, ammonia, and cyanogen, the observations were vitiated by these causes, and no proper values for the equivalent diffusion-volumes could be obtained for these gases.

Graham then points out that, admitting the law, the specific gravity of the gases may be determined by

the principle of diffusion; it is given by the formula $D = \left(\frac{A}{G}\right)^2$, where G is the volume of gas submitted to diffusion, and A the volume of the return air, and he proceeds to indicate a form of apparatus which may be conveniently employed for the purpose. As is well known, Bunsen has devised an instrument on the same principle, which obviates the use of stucco, and which is capable of affording results of a high degree of accuracy.

This "interchange in position of indefinitely minute volumes of gases" is a property inherent in the gases; inequality of density is not an essential requisite to diffusion. Graham proves this by connecting together two vessels, one containing nitrogen and the other carbon monoxide, which have the same density, by means of a short tube containing a stucco plug. The two vessels were allowed to remain connected together for about twenty-four hours, when the gases were found to be uniformly diffused through both vessels.

The relations of diffusion to evaporation and respiration are then indicated, and the memoir concludes with the statement that the "law," being merely a description of the appearances, involves no hypothesis, and "is certainly not provided for in the corpuscular philosophy of the day." It is altogether so extraordinary that Graham trusts he "may be excused for not speculating further upon its cause, till its various bearings, and certain collateral subjects, be fully investigated."

The path of inquiry thus opened up was pursued by Graham, with certain deviations to be afterwards pointed out, until the close of his life. Thirteen years, however, elapsed before his next important contribution to the subject made its appearance. In 1846 he sent to the Royal Society the first part of a long memoir on "The

Motion of Gases" (*Philosophical Transactions*, iv., 1846, pp. 573-632); the second part was not published until 1849 (*Philosophical Transactions*, ii., 1849, pp. 349-362). In this memoir he draws attention, at the outset, to the necessity of distinguishing between the passage of a gas through a small aperture in a thin plate, and its passage through a tube of sensible length. The phenomena of the first class are well-defined, simple, and in accordance with the law of diffusion; those of the second class were also regular in cases where the tubes were strictly "capillary," or where they were of great length, or, if short, of extremely small diameter. Capillary glass tubes varying in length from 20 feet to 2 inches were found equally available, and gave similar results, provided that sufficient resistance was offered to the passage of the gas. The result was independent of the nature of the material of the tube, and had no simple relation to the density of the gas. The two classes of phenomena are subject therefore to apparently essentially different laws. To mark the distinction, Graham terms the passage of gases through an aperture in a thin plate *effusion*, whilst its passage through a tube he calls *transpiration*. It is the object of his work to determine the coefficients of effusion and transpiration of various gases.

With respect to the effusion experiments Graham found that "*different gases pass through minute apertures into a vacuum in times which are as the square roots of their respective specific gravities, or with velocities which are inversely as the square roots of their specific gravities*—that is, according to the same law as gases diffuse into each other." In the case of a mixture of gases the rate was in strict accordance with the specific gravity of the mixture; thus in the

case of a mixture of equal volumes of carbonic oxide and oxygen the time of effusion was as the square root of the density of the mixture. As regards the effects of pressure on the effusion-rate of a gas no very definite results were obtained; doubling the density of the air by compression scarcely affected the time of effusion of equal volumes. Air at different temperatures has an effusion time proportional to the square root of its density at each temperature. "As the velocity of the effusion of air does not increase at a rate so rapid as the direct proportion of its expansion by heat, it follows that the flow of air through a small aperture is retarded by heating the air—that is, the same absolute quantity or weight of air will take a longer time to pass, when rarefied by heat, than when in a dense state."

The experiments on transpiration led to the following general conclusions:—

1. The velocities with which different gases pass through capillary tubes bear a constant relation to each other. This constancy of relation was observed for tube resistances varying in amount from 1 to 1000. These relations are apparently more simple in their expression than the densities of the gases, and seem to depend upon a peculiar and fundamental property of the gaseous form of matter.

The velocity of hydrogen is exactly double that of nitrogen and carbonic oxide.

The velocities of nitrogen and oxygen are inversely as the specific gravities of these gases.

The velocity of nitric oxide is the same as that of nitrogen and carbonic oxide.

The velocities of carbonic acid and nitrous oxide are equal, and when compared with oxygen directly proportional to their specific gravities.

The velocity of methane is 0·8 when that of hydrogen is 1.

The velocity of chlorine is $1\frac{1}{2}$ times that of oxygen; the vapours of bromine and sulphur trioxide have the same velocity as oxygen.

Ether vapour appears to have the same velocity as hydrogen gas.

Olefiant gas, ammonia, and cyanogen appear to have equal or nearly equal velocities, which approach closely to double the velocity of oxygen.

Sulphuretted hydrogen and carbon bisulphide vapour appear to have equal or nearly equal velocities.

2. The resistance of a capillary tube of uniform bore to the passage of any gas is directly proportional to the length of the tube.

3. The velocity of passage of equal volumes of air of the same temperature but of different densities is directly proportional to the density.

4. Rarefaction by heat exerts a similar and precisely equal effect in diminishing the velocity of the transpiration of equal volumes of air, as the loss of density by diminution of pressure.

It would appear, then, that transpiration is promoted by increase of density, and equally, no matter whether the increased density is due to compression, to cold, or to the addition of an element in combination; thus the velocity of oxygen is increased by combining it with carbon, which unites with it without change of volume to form carbonic acid.

Graham lastly points out that the distribution of coal gas in the mains of our cities must proceed in accordance with the laws of gaseous transpiration, for the reason that although the pipes may be many inches in diameter, their length is much beyond 4000 times

their width, the limiting ratio required in order that the flow shall be "capillary."

In a memoir "On the Molecular Mobility of Gases," which appeared in the *Philosophical Transactions* for 1863, Graham returns to the question of the passage of gases under pressure through a thin porous plate or septum, and to the partial separation of mixed gases, which can be effected by such means. He had discovered that a much better material than stucco for such a septum existed in the artificially-compressed graphite of Mr. Brockedon, which could be obtained in small cubic masses about two inches square, and from which slices of a millimetre or two in thickness could be readily cut by means of a saw of steel spring. By rubbing the surface of the slice without wetting it upon a flat sandstone, the thickness might be further reduced to about one-half of a millimetre. A wafer of this material could be readily affixed to the end of a tube by means of a resinous cement, and the whole constituted a diffusiometer which presented many points of advantage over the forms hitherto employed. Native graphite is of lamellar structure, and has little or no porosity: hence it cannot be substituted for the artificial variety as a diffusion-septum. "The pores of artificial graphite appear to be really so minute, that a gas *in mass* cannot penetrate the plate at all. It seems that molecules only can pass; and they may be supposed to pass wholly unimpeded by friction, for the smallest pores that can be imagined to exist in the graphite must be tunnels in magnitude to the ultimate atoms of a gaseous body. The sole motive agency appears to be that intestine movement of molecules which is now generally recognised as an essential property of the gaseous condition of matter."

Graham then points out that the rate of passage of different gases through a minute aperture in a thin plate is regulated by their specific gravities, according to a law which was deduced by Robison from Torricelli's well-known theorem of the velocity of efflux of fluids. "A gas rushes into a vacuum with the velocity which a heavy body would acquire by falling from the height of an atmosphere composed of the gas in question, and supposed to be of uniform density throughout. The height of the uniform atmosphere would be inversely as the density of the gas; the atmosphere of hydrogen, for instance, being sixteen times higher than that of oxygen. But as the velocity acquired by a heavy body in falling is not directly as the height, but as the square root of the height, the rate of flow of different gases into a vacuum will be inversely as the square root of their respective densities. The velocity of oxygen being 1, that of hydrogen will be 4, the square root of 16."

The memoir next deals with the consideration of the question of the diffusion of mixed gases into a vacuum, and several methods of effecting the atmolysis of mixed gases are described; and the paper concludes with an account of some experiments on the inter-diffusion of gases without an intervening septum.

By a natural association of ideas, easy to trace, Graham was led to pass from the study of the molecular movement of gases to that of liquids. His classical memoir "On the Diffusion of Liquids" formed the subject of the Bakerian Lecture to the Royal Society in 1849. It contains the first results of a lengthened inquiry into the rates of movement which substances, principally saline, exhibit in passing out of an aqueous solution into pure water. The method of experiment,

as in all Graham's investigations, was extremely simple. In its final form it consisted in placing a small phial, of about 4 oz. capacity, and of about $1\frac{1}{4}$ inches in diameter at the neck, on the bottom of a cylindrical glass jar partially filled with water. The phial was charged to within half an inch of the top with the saline solution to be examined, and the rest of the space was cautiously filled with pure water in such manner as not to disturb the saline solution. The phial was next covered with a glass plate, and water was poured into the outer vessel to the height of 1 inch above the plate, the amount of water thus needed being about 30 oz., *i.e.* more than seven times the volume of the saline solution. The glass plate was then carefully removed, and the "diffusion cell," as the entire apparatus may be fitly termed, was left to itself for a definite number of days at a constant temperature. At the expiration of the time the phial was again covered with the glass plate and withdrawn from the jar, and the amount of the salt which had passed into the outer liquid was determined either by evaporation or by analysis.

The results obtained by Graham from a large number of observations can only be briefly summarised here. In the first place it was found that the mere density of the solutions had no direct influence on the rate of diffusion. A number of aqueous solutions of acids and salts were prepared of the uniform density of 1·2, and these were allowed to diffuse in the manner described. It was found that the rates were very unequal, ranging from 1 to 0·1333, and evidently depended on the character of the substance. In the case of moderately dilute solutions (4 or 5 per cent), the amount of any one salt diffused in equal intervals of time is directly proportional to the quantity of the salt in the diffusing

solution. The quantity of salt diffused increases with, and is apparently in direct relation to, the temperature; but the proportionality in the diffusion with strength of the saline solution is maintained at different temperatures. The various classes of substances show the widest possible differences in rapidity of diffusion. Thus albumen is more than twenty times less diffusible than common salt. Cane-sugar and starch-sugar, which are equi-diffusive, have double the rate of gum-arabic, but less than half the rate of common salt. These great differences appeared to Graham to promise the possibility of a delicate method of proximate analysis specially applicable to animal and vegetable fluids. The low diffusibility of albumen is significant in connection with the retention of the serous fluids within the blood-vessels. A solution of albumen, in spite of its viscidity, does not impair the diffusion of salts which may be present with it in the solution. Sodium chloride, urea, and sugar are found to diffuse out quite as freely from the albuminous liquid as from an equal volume of pure water.

Isomorphous salts are equi-diffusive. Thus sal-ammoniac and potassium chloride diffuse at the same rate; so do nitre and ammonium nitrate; Epsom salts and white vitriol; and the nitrates of lime, strontia, and baryta. The relation observed is the more remarkable, in that it is of equal weights, and not of equi-molecular weights. Density of the aqueous solutions, in the case of the several pairs, or a difference in the solubility of the salts, appears to have no influence on the result. It must, however, be stated that equality or similarity of diffusion is not necessarily confined to the isomorphous groups of salts.

Hydrochloric, hydrobromic, and hydriodic acids,

which are among the most diffusive substances known, are also equi-diffusive, despite the differences in the boiling points and specific gravities of their solutions. Hence it appears that a considerable diversity of physical properties may be compatible with equal diffusibility. Hydrocyanic acid, on the other hand, is considerably less diffusive than the halogen hydracids. The alkaline iodides, bromides, and chlorides, when strictly isomorphous, are also equi-diffusive.

In the case of a solution containing a mixture of salts, which, so far as is known, exert no chemical action on each other, it might be anticipated that each salt would diffuse separately and independently, according to its special rate. Experiments with mixtures of two salts showed that the less soluble of the two diffused more slowly than when in the unmixed state. This is true even in the case of a mixture of two isomorphous salts, which when diffused singly were equi-diffusive.

Inequality of diffusion may evidently be made the basis of a method of separating, to a greater or less extent, certain salts from one another, just as unequally volatile substances may be separated by fractional distillation. For example, the potash salts are invariably more diffusive than the corresponding soda salts; hence it follows that if a solution of the mixed salts be placed within the phial, the potash salt will escape in larger proportion into the outer liquid, whilst the soda salt will be relatively concentrated within the phial. Even sea-water, when subjected to this treatment, gives up its saline contents in very different proportions. Graham sees in this behaviour a possible explanation of the discrepancies in the analyses which have been

published from time to time of the water of the Dead Sea, in which the relative proportions of the various constituents are very discordant. "The lake in question," he says, "falls in level 10 or 12 feet every year by evaporation. A sheet of fresh water of that depth is thrown over the lake in the wet season, which water may be supposed to flow over a fluid nearly 1·2 in density without greatly disturbing it. The salts rise from below into the superior stratum by the diffusive process, which will bring up salts of the alkalies with more rapidity than salts of the [alkaline] earths, and chlorides of either class more rapidly than sulphates. The composition of water near the surface must therefore vary greatly as this process is more or less advanced."

This process of liquid diffusion is capable of throwing much light on the question of the nature and constitution of salts when dissolved. Graham found, for example, that a solution of acid sulphate of potassium behaved like a mixture of the normal salt and free sulphuric acid. It was possible, in fact, to effect an almost complete separation of the two components. Common potash-alum, in like manner, on solution was apparently resolved into potassium sulphate and aluminium sulphate. Other so-called double salts were found to be decomposed in like manner; thus Rochelle salt was found to be resolved into the tartrates of potash and soda.

The question whether a double salt is formed at once when its constituent salts are dissolved together, or is produced only in the act of crystallisation, may therefore be answered. It appears that such double salts are not necessarily formed immediately on solution of their constituent salts. As Graham points out,

many practices in the chemical arts which seem empirical may have their foundation in facts of this kind, as, for example, the manufacture of potassium chloride from carnallite.

The investigation of the diffusion of one salt into the solution of another salt has a special interest, as serving to indicate how far the phenomena of liquid and gaseous diffusion are really analogous. In the case of gases it is found that oxygen, for example, freely diffuses into the space already occupied by hydrogen, whilst hydrogen, in return, passes with equal freedom into the space occupied by oxygen. Does anything precisely analogous to this occur with saline solutions? In the case of dilute solutions, at least, observation shows there is the very closest resemblance. The presence of one salt exercises no resistance to, or interference with, the diffusion of the other. To use Graham's phrase, "salts are therefore inelastic to each other, like two different gases."

It is not at all improbable that these results may be greatly modified in concentrated solutions. "There is," says Graham, "reason to apprehend that the phenomena of liquid diffusion are exhibited in the simplest form by dilute solutions, and that concentration of the dissolved salt, like compression of a gas, is attended often with a departure from the normal character. On approaching the degree of pressure which occasions the liquefaction of a gas, an attraction appears to be brought into play which impairs the elasticity of the gas; so on approaching the point of saturation of a salt, an attraction of the salt molecules for each other, tending to produce crystallisation, comes into action, which will interfere with and diminish that elasticity or dispersive tendency of the dissolved salt which occasions its dif-

fusion. . . . The analogy of liquid diffusion to gaseous diffusion and vaporisation is borne out in every character of the former which has been examined. Mixed salts appear to diffuse independently of each other, like mixed gases, and into a water atmosphere already charged with another salt as into pure water. Salts also are unequally diffusible, like the gases, and separations both mechanical and chemical (decompositions) are produced by liquid as well as by gaseous diffusion."

In concluding his memoir Graham points out that "the fact that the relations in diffusion of different substances refer to equal weights of those substances, and not to their atomic [molecular] weights or equivalents, is one which reaches to the very basis of molecular chemistry. The relation most frequently possessed is that of equality, the relation of all others most easily observed. In liquid diffusion we appear to deal no longer with chemical equivalents or the Daltonian atoms, but with masses even more simply related to each other in weight. Founding still upon the chemical atoms, we may suppose that they can group together in such numbers as to form new and larger molecules of equal weight for different substances, or, if not of equal weight, of weights which appear to have a simple relation to each other. It is this new class of molecules which appears to play a part in solubility and liquid diffusion, and not the atoms of chemical combination."

In a short paper, communicated to the Chemical Society in 1851, Graham gives the following table, showing the amount of

SALT DIFFUSED FROM A 1 PER CENT SOLUTION IN EQUAL TIMES.

	Grains.		Grains.
Potash	4·84	Sodium sulphate	2·35
Soda	4·03	Calcium chloride	2·02
Ammonium chloride	3·42	Magnesium chloride	2·03
Potassium chloride	3·42	Calcium sulphate	1·21
Sodium chloride	2·85	Magnesium sulphate	1·21

These are the latest, and perhaps the best determined, diffusibilities observed by Graham, and serve to confirm many of the results already referred to. They show, in the first place, that potash compounds are more diffusive than the corresponding compounds of soda; and, in the second, that isomorphous compounds are equi-diffusive.

The rest of the paper is concerned with a description of experiments on the decomposition of the sulphates of soda and potash, and of the chlorides of potassium and sodium by means of lime, in which it may be expected that the affinity of that base for an acid is aided by the high diffusibility of the potash or soda. It was found that both the sulphates were decomposed, and that the alkaline hydrates made their appearance in the liquid in the outer vessel at rates dependent on their specific diffusibility, whereas the calcium sulphate formed, on account of its low diffusive power, only slowly escaped from the phial, where, indeed, it was deposited in crystals. The decomposition of potassium sulphate by lime, with deposition of calcium sulphate, was observed by Scheele, and explained by Berthollet as dependent on the insolubility of the latter salt.

A solution of calcium carbonate in water charged with carbonic acid is also capable of decomposing the sulphates of potassium and sodium, but to a less extent than lime alone, and without deposition of calcium sulphate. The alkaline chlorides are not decomposed

by lime-water, nor by a solution of chalk. On the other hand, a mixture of lime-water and calcium sulphate solution, *when boiled with the chlorides*, gave diffusates containing free alkalies. This is explained by Graham as due to the transformation of the alkaline chloride into alkaline sulphate, which is then decomposed by the lime in the manner already indicated.

Graham ends his paper by a reference to a possible bearing of liquid diffusion on agriculture which is not without interest. The mode in which the soil is moistened by rain is, he says, "peculiarly favourable to separations by diffusion. The soluble salts of the soil may be supposed to be carried down together, to a certain depth, by the first portion of rain which falls, while they find afterwards an atmosphere of nearly pure water, in the moisture which falls last and occupies the surface stratum of the soil. Diffusion of the salts upwards into the water, with its separations and decompositions, must necessarily ensue. The salts of potash and ammonia, which are most required for vegetation, possess the highest diffusibility, and will rise first. The pre-eminent diffusibility of the alkaline hydrates may also be called into action in the soil by hydrate of lime, particularly as quicklime is applied for a top-dressing to grass lands."

The remarkable differences in the diffusive power of substances naturally led Graham to consider the practicability of employing the process as an analytical agent, and in a paper published in the *Philosophical Transactions* for 1861 he describes how liquid diffusion may be so made use of. To begin with, he draws attention to the fact that soluble substances may be broadly divided into two main classes dependent on their diffusibility. The one class, of low diffusibility,

is characterised by the absence of the power to crystallise. Among such substances Graham mentions "hydrated silicic acid, hydrated alumina, and other metallic peroxides of the aluminous class, when they exist in the soluble form, with starch, dextrin, and the gums, caramel, tannin, albumen, gelatine, and vegetable and animal extractive matters." Low diffusibility is not the only property which these bodies possess in common. "They are distinguished by the gelatinous character of their hydrates. Although often largely soluble in water, they are held in solution by a most feeble force. They appear singularly inert in the capacity of acids and bases, and in all the ordinary chemical relations. But, on the other hand, their peculiar physical aggregation, with the chemical indifference referred to, appears to be required in substances that can intervene in the organic processes of life. The plastic elements of the animal body are found in this class. As gelatine appears to be its type, it is proposed to designate substances of this class as *colloids*, and to speak of their peculiar form of aggregation as the *colloidal condition of matter*."

The other class, of relatively high diffusive power, is broadly characterised by the power to crystallise. "Substances affecting the latter form will be classed as *crystalloid*. The distinction is no doubt one of intimate molecular constitution."

"Although chemically inert in the ordinary sense, colloids possess a compensating activity of their own, arising out of their physical properties. While the rigidity of the crystalline structure shuts out external impressions, the softness of the gelatinous colloid partakes of fluidity, and enables the colloid to become a medium for liquid diffusion, like water itself. The same

penetrability appears to take the form of cementation in such colloids as can exist at a high temperature. . . . Another and eminently characteristic quality of colloids is their mutability. Their existence is a continued metastasis. . . . The colloidal is, in fact, a dynamical state of matter; the crystalloidal being the statical condition. The colloid possesses ENERGIA. It may be looked upon as the probable primary source of the force appearing in the phenomena of vitality. To the gradual manner in which colloidal changes take place (for they always demand time as an element), may the characteristic protraction of chemico-organic changes also be referred."

Liquid diffusion may be made use of for analytical purposes, either with or without an intervening septum. In the latter case the solution of the substance to be submitted to diffusive separation is brought, by means of a fine pipette, to the bottom of a column of water some 5 or 6 inches high, and after some days the upper layers of the water are syphoned off, and the dissolved matter recovered by evaporation or other suitable process.

A more generally applicable method, however, is to use a septum; to such a mode of separation Graham applies the convenient term *dialysis*. The most suitable of all substances for the dialytic septum he found to be what is known as vegetable-parchment or parchment-paper, prepared by immersing unsized paper in oil of vitriol or zinc chloride solution. This is stretched between two concentric and tightly-fitting hoops of gutta-percha, so as to form a tambourine-shaped vessel or *dialyser* capable of floating on the surface of water. The mixed fluid to be dialysed is poured over the parchment-paper to the depth of about

half an inch, and the *dialyser* is then floated upon water contained in a basin.

The first method has the particular advantage that it affords the means of ascertaining the absolute rate or velocity of diffusion. It thus becomes possible to state the distance which a salt travels per second in terms of some unit of length. "It is easy to see," says Graham, "that such a constant must enter into all the chronic phenomena of physiology, and that it holds a place in vital science not unlike the time of the falling of heavy bodies in the physics of gravitation."

Graham's observations lead to the following relative

Approximate Times of Equal Diffusion.

Hydrochloric acid	1	Magnesium sulphate	7
Sodium chloride	2·33	Albumen	49
Sugar	7	Caramel	98

A question of the greatest importance in regard to the real nature of solution was raised by Graham in studying the effect of the acid in influencing the diffusibility of basic radicles. If the acids are of equal diffusibility, there seems no reason why the acids should affect the amount of separation. "But if," says Graham, "the acids are unlike in diffusibility, the case is not so clear. If, for instance, the potassium were in the form of chloride, and the sodium in that of sulphate, might not the diffusion of the potassium be promoted by the highly diffusive chlorine with which it is associated, and the diffusion of the soda, on the other hand, be retarded by its association with the slowly diffusive sulphuric acid? Will, in fine, the separation of the metals be greater from a mixture of chloride of potassium and sulphate of soda, or even from sulphate of potash and chloride of sodium, than from the two

chlorides or from the two sulphates? The inquiry, it will be remarked, raises the whole question of the distribution of acid and base in solutions of mixed salts." The answer given by Graham to this question is very significant. It was found that the diffusion of the basic radicles was not affected by the acid with which they were considered to be combined. This result is in harmony, as Graham points out, with Berthollet's view that the acids and bases are indifferently combined, or that a mixture of sodium sulphate and potassium chloride is the same thing as a mixture of potassium sulphate and sodium chloride when the mixtures are in a state of solution.

As regards the second or dialytic method, it was found that the diffusive process was but slightly interfered with by the intervention of the septum. Provided that it was a true colloid, the nature of the septum—whether gelatinous starch, coagulated albumen, gum-tragacanth, or parchment-paper—had no influence on the result. The relative diffusibility of salts seemed to be unchanged; mixed salts were separated in exactly the same manner as in the absence of the septum, and so-called double salts were found to be decomposed and their proximate constituents to part company as already described.

The method of dialysis is admirably adapted to the preparation of colloid substances in a state of purity; and not the least interesting part of Graham's memoir is concerned with the description of the properties of a number of these singular bodies, which were obtained in this manner. Thus when a solution of silica, obtained by pouring silicate of soda into a large excess of dilute hydrochloric acid, is placed on a dialyser of parchment-paper, the sodium chloride, together with the rest of the

hydrochloric acid, passes through the septum, whilst a pure solution of *silicic acid* remains. This may be concentrated by boiling in a flask, and forms a perfectly limpid colourless liquid, having little viscidity, even when containing 14 per cent of silicic acid. It gradually, however, becomes opalescent, and eventually forms a transparent colourless jelly, which slowly contracts and hardens. The coagulation of the silicic acid is rapidly effected by a minute trace of any alkaline or earthy carbonate, but not by caustic ammonia, nor by neutral or acid salts. The common acids, alcohol or sugar seem to have no coagulating effect. A trace of hydrochloric acid, or of potash or soda, appears to render the solution more stable. The pure solution has an acid reaction, greater than that of carbonic acid. The jelly, when dried *in vacuo*, forms a transparent, vitreous, insoluble mass of considerable lustre. Certain colloids, as gelatin, alumina, and ferric oxide, precipitate the silicic acid ; gum-arabic and caramel appear to have no action. The precipitates seem to be weak chemical combinations ; the gelatin compound is a flaky, white, and opaque substance, insoluble in water, and not decomposed by washing. It may be obtained containing almost half its weight of gelatin. The proportion, however, is not constant, but varies with the mode of preparation. Graham, in a subsequent paper, describes combinations of colloidal silicic acid with ethyl alcohol, ether, benzene, carbon bisulphide and glycerin.

The existence of a soluble form of *alumina* was known before the publication of Graham's paper. It was discovered by Mr. Walter Crum, and is obtained by heating a solution of alumina acetate, when the whole of the acetic acid is expelled, whilst the alumina

remains dissolved. According to Graham, another modification of soluble alumina may be obtained by dialysing a solution of basic aluminium chloride. Normal aluminium chloride passes through the septum, and eventually a solution of colloidal alumina is left on the dialyser. The solution is very unstable, and is coagulated by minute traces of many foreign substances. This form of alumina acts as a mordant; its solution may be concentrated by boiling, but is apt to coagulate suddenly. It has a feeble alkaline reaction, and is readily dissolved by acids. Meta-alumina, as Graham terms Crum's modification, has no mordanting action, and when once precipitated is not dissolved by an excess of acid.

A similar colloidal form of *ferric hydrate* may also be obtained by dialysing basic ferric chloride solution. A liquid remains on the dialyser, of the dark red colour of venous blood; an aqueous solution containing only 1 per cent of the colloidal ferric oxide has the colour of blood. It may be concentrated to a certain extent, but readily coagulates to a deep red jelly, somewhat resembling a clot of blood. Graham considers that native hæmatite, which is found in mammillary concretions, is in all probability colloidal. A soluble ferric hydrate, analogous to the meta-alumina of Crum, was obtained by Péan de Saint-Gilles by the action of heat upon ferric acetate. Its solution has an orange-red colour, and is more or less opalescent.

Chromic hydrate exists in similar colloidal modifications. *Copper ferrocyanide* also appears to be a colloid, and may be obtained in a soluble form by dialysing its solution in ammonium oxalate. The greater part of the latter salt passes through the parchment-paper, and there is left a red solution of the

ferrocyanide, which may be heated without change, but which is coagulated by foreign substances with great readiness.

A colloidal modification of *Prussian blue* and of *Turnbull's blue* may be obtained by dialysing the oxalic acid solutions of these colouring matters.

The blue liquid obtained by adding caustic potash to a solution of cupric chloride and sugar also contains a colloidal substance; it consists apparently of cupric oxide combined with sugar, and is readily precipitated by salts and acids. Similar compounds of sugar with ferric and uranic hydrates appear to exist.

Stannic and titanic hydrates are soluble in aqueous solutions of their chlorides, or in hydrochloric acid, but when these are submitted to dialysis they yield the hydrates in semi-transparent gelatinous cakes. Colloidal modifications of tungstic and molybdic acids are also known.

Dialysis has proved of much service in toxicological investigations, where the poisonous substance has to be searched for in fluid mixtures containing, it may be, a considerable amount of colloidal matter. The fluid, diluted, if necessary, with water, is placed on the dialyser, when such poisons as arsenious acid, tartar-emetic, and strychnine pass through the parchment-paper, and may be detected in the diffusate. The process has the great advantage of introducing no metallic substance or chemical reagent into the organic fluid from which the poison has to be separated.

Graham concludes his memoir with a short summary of the general characters of the colloidal condition of matter, and with some interesting and suggestive observations on osmose. He points out that a radical distinction in intimate molecular constitution must

exist between crystalloids and colloids. "Every physical and chemical property is characteristically modified in each class. They appear like different worlds of matter, and give occasion to a corresponding division of chemical science. The distinction between these kinds of matter is that subsisting between the material of a mineral and the material of an organised mass.

"The colloidal character is not obliterated by liquefaction, and is therefore more than a modification of the physical condition of solid." In their relations to water—*e.g.* solubility, power of combination—the colloids exhibit as great diversity as the crystalloids. "The phenomena of the solution of a salt or crystalloid probably all appear in the solution of a colloid, but greatly reduced in degree. . . . The change of temperature usually occurring in the act of solution becomes [in the case of the colloid] barely perceptible. The liquid is always sensibly gummy or viscous when concentrated. The colloid, although often dissolved in a large proportion by its solvent, is held in solution by a singularly feeble force. Hence colloids are generally displaced and precipitated by the addition to their solution of any substance from the other class. Of all the properties of liquid colloids, their slow diffusion in water, and their arrest by colloidal septa, are the most serviceable in distinguishing them from crystalloids. Colloids have feeble chemical reactions, but they exhibit at the same time a very general sensibility to liquid reagents, as has already been explained.

"While soluble crystalloids are always highly sapid, soluble colloids are singularly insipid. It may be questioned whether a colloid, when tasted, ever reaches the sentient extremities of the nerves of the palate, as the latter are probably protected by a colloidal

membrane impermeable to soluble substances of the same physical constitution.

"It has been observed that vegetable gum is not digested in the stomach. The coats of that organ dialyse the soluble food, absorbing crystalloids and rejecting all colloids. This action appears to be aided by the thick coating of mucus which usually lines the stomach. . . .

"*Ice* itself presents colloidal characters at or near its melting-point, paradoxical although the statement may appear. When ice is formed at temperatures a few degrees under 0° C., it has a well-marked crystalline structure, as is seen in water frozen from a state of vapour, in the form of flakes of snow and hoar-frost, or in water frozen from dilute sulphuric acid, as observed by Mr. Faraday. But ice formed in contact with water at 0° is a plain homogeneous mass, with a vitreous fracture, exhibiting no facets or angles. This must appear singular when it is considered how favourable to crystallisation are the circumstances in which a sheet of ice is slowly produced in the freezing of a lake or river. . . . Further, ice, although exhibiting none of the viscous softness of pitch, has the elasticity and tendency to rend, seen in colloids. In the properties last mentioned, ice presents a distant analogy to gum incompletely dried, to glue, or any other firm jelly. Ice further appears to be of the class of adhesive colloids. The redintegration (regelation of Faraday) of masses of melting ice, when placed in contact, has much of a colloid character. A colloidal view of the plasticity of ice demonstrated in the glacier movement will readily develop itself."

The osmotic process, which is obviously closely connected with the action of colloidal septa, appears

to Graham to depend, so far as regards the water movement, on the hydration and dehydration in the substance of the membrane or other colloidal septum; the diffusion of the saline solution placed within the osmometer has little or nothing to do with the osmotic result otherwise than as it affects the state of hydration of the septum. An animal membrane is much affected by the liquid medium in which it is placed, and is hydrated to a higher degree by pure water than by neutral saline solutions. "Hence the equilibrium of hydration is different on the two sides of the membrane of an osmometer. The outer surface of the membrane, being in contact with pure water, tends to hydrate itself in a higher degree than the inner surface does, the latter surface being supposed to be in contact with a saline solution. When the full hydration of the outer surface extends through the thickness of the membrane and reaches the inner surface, it there receives a check. The degree of hydration is lowered, and water must be given up by the inner layer of the membrane, and it forms the osmose."

The conditions affecting the flow of liquids through narrow tubes next engaged Graham's attention. He had shown that a gas passes through a capillary tube in a manner altogether different from that in which it passes through an orifice in an extremely thin plate, or into an atmosphere of another gas without the intervention of a septum. In other words, the *transpiration* and the *diffusion* of gases are governed by altogether different laws. In view of the analogy existing between the diffusion of a gas and a liquid, it became of interest to study the transpiration of liquids. This subject had already been attacked by Poiseuille, who had ascertained the experimental con-

ditions under which comparative measurements can be accurately made, and had determined the relations between the rate of flow, the dimensions of the capillary tube, and the pressure under which the transpiration is effected. Poiseuille's measurements were, however, confined to a few liquids standing to one another in no definite chemical relationship. Graham sought to extend the inquiry, and his memoir "On the Capillary Transpiration of Liquids in relation to Chemical Composition," communicated to the Royal Society in 1861, contains the results of the determination of the transpiration rates of a number of substances. The method adopted was essentially that of Poiseuille, in which the time required for a given volume of the liquid to flow through a capillary of known dimensions under a definite pressure is noted. Graham made his measurements at the uniform temperature of 20°, under the assumption, apparently, that this was a truly comparable condition. No definite results could possibly follow from such a method of inquiry, and indeed there is reason to believe that he merely regarded his observations as preliminary to a much wider investigation. The main object, in the outset, was to ascertain the existence of definite hydrates of the acids by noting the effect of the added water on the times of flow, Poiseuille having already found that the addition of water to ethyl-alcohol gradually retarded the transpiration up to a certain point, apparently corresponding with a combination of one molecule of alcohol with three molecules of water. Although the existence of such hydrates seemed to be indicated in a few cases, in others the observations were equivocal. A number of measurements were made on the pure alcohols, acids, and on mixed ethers, from which Graham inferred that the

increase of the transpiration time, as the series is ascended in each particular case, is connected with the increasing weight of the molecule. The general conclusion is that a relation between the transpirability of liquids and their chemical composition undoubtedly exists. "It is a relation analogous in character to that subsisting between the boiling-point and composition, so well defined by M. Kopp."

Graham continued to speculate and to work on the subject of the molecular movement of substances until his death; and no mode of attack that occurred to him as likely to furnish fresh light on the great problem which had occupied his thoughts during the whole of his scientific life was neglected. The remarkable observation by Dr. Mitchell of Philadelphia, made in 1830, that unvulcanised india-rubber absorbs the various gases in very different degrees, was the occasion of the memoir "On the Absorption and Dialytic Separation of Gases by Colloid Septa," which Graham communicated to the Royal Society in 1866. It is interesting to note in this paper the tenacity with which he clings to the ideas which had actuated him in his earliest investigations. Indeed this was characteristic of the man. Graham's convictions were not lightly formed; often the result of prolonged and patient thought, when once arrived at they were, as in the case of most independent thinkers, adhered to with a firmness which occasionally verged on obstinacy. Dr. Mitchell's observations "On the Penetrativeness of Fluids," as his memoir is entitled, led Graham to the generalisation that those gases penetrate most readily which are easily liquefied by pressure, and which are also "generally highly soluble in water or other liquids." "Two considerations," he says, "appear to be essential to the full comprehension

of the phenomena—namely, that gases undergo liquefaction when absorbed by liquids and such colloid substances as india-rubber, and that their transmission through liquid and colloid septa is then effected by the agency of liquid, and not gaseous diffusion. Indeed the complete suspension of the gaseous function during the transit through colloid membranes cannot be kept too much in view."

It must be admitted that Graham's own experimental determinations lend little support to his hypothesis of the nature of the process by which gases pass through rubber. The order of "penetrativeness" exhibits no exact relation either to the solubility of the gases in ordinary solvents or to their coercibility. This will be evident from the following table, showing the

Penetration of Rubber by Equal Volumes of Gas.

	Time.
Carbonic acid	1
Hydrogen	2·470
Oxygen	5·316
Marsh gas	6·325
Atmospheric air	11·850
Carbonic oxide	12·203
Nitrogen	13·585

Carbonic acid, the most easily liquefied in the series, and the most soluble of the whole, certainly comes first; but it is followed by hydrogen, which is the only one of the group which has resisted all attempts to effect its liquefaction, and which is the least soluble in water. Nor do the other gases follow in the order demanded by Graham's supposition. If, as Graham argues, the absorption of the gas by rubber depends upon a kind of chemical affinity analogous to the attraction which appears to exist between a soluble body and its solvent,

conducing to solution, it has still to be proved that rubber is so peculiar in its action that the order of solubility is altogether different from that exhibited by other solvents. In the light of our present knowledge it is even more difficult to assume that an actual liquefaction of hydrogen takes place.

Graham found that the penetrability of rubber was much affected by temperature, the film becoming more and more permeable as it becomes heated. This, at first sight, seems to militate against his hypothesis, since increase of temperature tends to prevent liquefaction. Graham, however, seeks an explanation in the tendency of the colloid to become more soft when heated, and, as he expresses it, "to acquire more of liquid and less of solid properties."

Owing to the very different rates at which oxygen and nitrogen pass through rubber—the "penetrativeness" of oxygen being more than two and a half times that of nitrogen—Graham found that it was possible to effect what he terms a dialytic separation of the two gases from atmospheric air. This may be accomplished by filling a balloon of thin rubber—one of the toy-balloons of the shops answers admirably—with hydrogen or carbonic acid, and after the expiration of a few hours, when the balloon will be found to have become less distended, analysing the gaseous residue. In one experiment with hydrogen the ratio of the oxygen to the nitrogen in the gaseous residue was as 41·6 per cent to 58·4 per cent. With carbonic acid the dialytic separation can be very readily demonstrated, since the residual carbonic acid may be quickly removed from the gaseous mixture by means of a solution of caustic potash or soda, when, under favourable circumstances, it will be found that the remaining gas contains sufficient

oxygen to inflame a glowing splint. Bags or balloons of india-rubber, or long lengths of vulcanised rubber-tubing, attached to the Sprengel pump, may also be made use of to show this dialytic separation. The bag, or length of tubing (the latter closed, of course, at the unattached end), is rapidly exhausted in the usual way, and the gas, which thereafter slowly permeates the rubber, is collected at the bottom of the fall-tube. This gas readily rekindles a glowing splint, and on analysis is found to contain from 38 to 41 per cent by volume of oxygen, the amount depending on the temperature and the nature of the septum; but in any case a very notable increase on the 21 per cent in the undialysed air.

The analogy which these facts exhibit to the passage of gases, and more particularly of hydrogen, through heated plates of platinum or iron, as observed by Deville and Troost, led Graham to experiment on the action of metallic septa at a red heat. He seems, however, to be sensible that this analogy lends no very strong support to his views as to the real nature of the penetrative process. "It must be admitted," he says, "that such an hypothesis as that of liquefaction can only be applied in a general and somewhat vague manner to bodies so elastic and volatile at an elevated temperature as the gases generally must be, and hydrogen in particular. Still some degree of absorbing and liquefying power can scarcely be denied to a soft or liquid substance, in whatever circumstances it may be found, with such a patent fact before us as the retention by fused silver of 18 or 20 volumes of oxygen at a red heat. It may be safely assumed that the tendency of gases to liquefaction, however much abated by temperature, is too essential a property of matter to be ever entirely obliterated." He seeks to strengthen his conjecture by an appeal to

analogy. "The absorption of gas by a liquid or by a colloid substance," he says, "is not a purely physical effect. The absorption appears to require some relation in composition—as where both the gas and the liquid are hydro-carbons, and the affinity or attraction of solution comes into play. May a similar analogy be looked for, of hydrogen to liquid, or colloid bodies of the metallic class?" The analogy is ingenious and even daring, but is not the courage that of a forlorn hope? Graham, it may be remarked, seldom trusts himself to the guidance of analogy alone; like Davy, he was aware how fruitful a parent of error it may be. To argue that the penetrability of hydrogen through platinum or iron is remotely due to the so-called metallic nature of hydrogen, is surely straining the analogy to the utmost limit. Graham would not apply the same reasoning to explain the solubility of oxygen in silver.

A large number of experiments were made to ascertain whether other gases than hydrogen would pass through red-hot platinum, but with negative results. Carbonic acid, which shows the greatest power of passing through rubber, was found to be incapable of permeating heated platinum. Ammonia gas and vapour of water—easily coercible substances—are also unable to penetrate the red-hot metal.

The greater part of the rest of the memoir is devoted to a study of the behaviour of hydrogen towards platinum and other metals. Graham found that hydrogen was not only absorbed by the red-hot platinum, but that it could be retained at a temperature under redness for an indefinite time. "It may be allowable to speak of this as a power to occlude (to shut up) hydrogen, and the result as the *occlusion* of hydrogen by platinum."

The power of finely divided platinum—especially the so-called platinum black—to absorb hydrogen was already known, but no exact determinations of the amount so absorbed, or any study of the conditions of retention, had been hitherto made. Graham found that the volume of gas absorbed, even under the most favourable conditions, was never more than about five times that of the metal, and was usually much less.

Of all the metals investigated, palladium appears to possess the power of absorbing hydrogen in the highest degree. Indeed its capacity in this respect is altogether peculiar. Graham found that welded palladium absorbs hydrogen to the extent of 600 times its volume at a temperature below the boiling-point of water; at 245° upwards of 500 volumes are absorbed. Even at ordinary temperatures a considerable quantity of the gas is occluded. On the other hand, palladium charged with hydrogen at or under 100° begins to evolve gas when exposed to the air or placed *in vacuo* at the original temperature of absorption; at 200° the gas is freely disengaged.

When palladium charged with hydrogen is left exposed to the atmosphere, the metal is apt to become suddenly hot, and to lose its gas entirely by spontaneous oxidation. A wire of the charged metal, if rubbed with magnesia (to make the flame luminous), burns like a waxed thread when ignited in the flame of a lamp.

The condensed hydrogen, as might be anticipated, is chemically active. A palladium wire charged with hydrogen, and immersed in a solution of a ferric salt, reduces it to the state of a ferrous salt; potassium ferricyanide becomes ferrocyanide; chlorine water forms hydrochloric acid; and iodine becomes hydriodic acid. Solutions of mercuric chloride, of certain oxy-salts, and

of vanadic acid are also found to be reduced by the occluded hydrogen. In respect to its chemical activity the condensed hydrogen is related to ordinary hydrogen much as ozone is related to oxygen.

Graham sees in these facts a strong confirmation of the validity of his hypothesis as to the real nature of the phenomenon. "It is probable," he says, "that hydrogen enters palladium in the physical condition of liquid, whether the phenomenon proves to be analogous to the imbibition of ether, chloroform, and such solvents, by the colloid india-rubber, or whether a certain porosity of structure in the palladium is required. The porosity of the metal is supposed to be of that high degree which will admit liquid but not gaseous molecules."

Graham further found that when coal-gas was led round the outside of a palladium tube attached to, and made vacuous by, a Sprengel pump, it was possible, on heating the tube to 270°, to sift out the hydrogen, to the exclusion of the other gases. Indeed the isolation is so exact that Graham considers that a quantitative determination of the hydrogen in a gaseous mixture might be based on this principle. It is noteworthy that the only other volatile body which was observed to pass through a plate of palladium is common ether: the vapour of this substance permeated palladium even at ordinary temperatures.

Osmium-iridium and antimony have no power to absorb hydrogen; copper, gold, silver, and iron absorb only a small amount, seldom exceeding half the volume of the metal. Silver, however, is remarkable for its power of absorbing oxygen, and iron for its capacity for retaining carbonic oxide. Graham found that pure iron is capable of taking up at a low red heat, and holding when cold, upwards of four times its volume of

carbonic oxide. This fact, he thinks, has an important bearing upon the process of steel-making, or *acieration*, by the cementation process. "Hitherto," he says, "the decomposing action of the iron upon carbonic oxide has been supposed to be exercised only at the external surface of the metal. A surface-particle of the iron has been supposed to assume one half of the carbon belonging to an equivalent of carbonic oxide [2 CO], while the remaining elements diffused away into the air as carbonic acid (CO_2) to reacquire carbon from the charcoal placed near, and to become capable of repeating the original action. It is now seen that such a process need not be confined to the surface of the iron bar, but may occur throughout the substance of the metal, in consequence of the prior penetration of the metal by carbonic oxide. . . . It appears that the diffused action of carbonic oxide is the proper means of distributing the carbon through the mass of iron. The blistering of the bar appears to testify to the necessary production and evolution of carbonic acid, owing to the decomposition of the carbonic oxide in the interior of the bar."

This memoir, although containing many important and suggestive facts, and of classical interest as giving the first intimation of the remarkable and peculiar capacity of palladium for occluding hydrogen, is hardly on a par with Graham's earlier papers in point of literary merit. It seems to have been somewhat hurriedly put together, and has in parts the appearance of being a mere transcript of a laboratory journal; the language is occasionally obscure, and there is a certain awkwardness of style, in striking contrast to that of the greater number of the memoirs which its author communicated to the *Philosophical Transactions*. That Graham was conscious of this may be inferred from the

many corrections and deletions which were found to have been made in his own copy; these are indicated in the late Dr. Young's reprint of his memoirs. There is no doubt that during its composition he was greatly worried with the business of the Mint; its affairs occupied, practically, the whole of his thoughts at that time, and consumed much of his nervous energy.

The remarkable observation, made in 1867, that the Lenarto meteorite when heated in a vacuum yielded gas to the extent of nearly three times its volume, 86 per cent of which gas was hydrogen, whereas ordinary iron when treated in the same manner evolved a relatively large proportion of carbonic oxide, was held by Graham to indicate that the iron of the Lenarto meteorite had been "extruded from a dense mass of hydrogen gas, for which we must look beyond the light cometary matter floating about within the limits of the solar system. . . . This meteorite may be looked upon as holding imprisoned within it, and bearing to us, hydrogen of the stars." It has been assumed that Graham regarded the presence of occluded hydrogen as characteristic of iron of extra-terrestrial origin, but there is nothing in his memoir, in the *Proceedings of the Royal Society* (vol. xv. p. 502, 1867), to justify such a supposition. Subsequent observations have shown that many irons of undoubted meteoric origin evolve considerable quantities of carbonic oxide and relatively little hydrogen.

In a subsequent paper, published in the *Proceedings of the Royal Society* for 1868, Graham states that a ready method of charging the palladium with hydrogen consists in making it the negative electrode of a voltaic couple immersed in acidulated water. Under these circumstances it is observed that whilst oxygen is

freely evolved at the positive electrode, the effervescence at the negative electrode is entirely suspended for some twenty or thirty seconds, in consequence of the hydrogen being occluded by the palladium, to the extent of several hundred times its volume. Although the hydrogen in all probability pervades the whole mass of the metal, the gas exhibits no disposition to leave the palladium, and to escape into a vacuum at the temperature of its absorption. "It appears then," says Graham, "that when hydrogen is absorbed by palladium, the volatility of the gas may be entirely suppressed; and hydrogen may be largely present in metals without exhibiting any sensible tension at low temperatures. Occluded hydrogen is certainly no longer a gas, whatever may be thought of its physical condition." By reversing the position of the palladium in the cell, so as, when charged, to make it the positive electrode, the hydrogen is rapidly withdrawn from the metal. This would seem to suggest that the palladium is charged mainly at the surface; otherwise it is not easy to understand how the oxygen, which has no power, at all events in the ordinary molecular condition, of permeating the metal, acts in withdrawing the hydrogen. Graham, moreover, found that not the least trace of oxygen was absorbed by palladium in the position of a positive electrode. Hence electrolytic oxygen, even in the moment of its liberation, shows no more tendency to pass into palladium than does the gas in the ordinary condition.

It is noteworthy that recent inquiries have rendered it certain that the layers of gases in immediate contact with solid surfaces are in a highly condensed state, and it is possible that this highly condensed gas possesses a degree of chemical activity which the gas in its

ordinary or more attenuated condition fails to exhibit.

The power of platinum-sponge to ignite a jet of hydrogen, as seen in the well-known Döbereiner lamp, depends, according to Graham, upon the influence of the metal on the occluded hydrogen. The hydrogen, he thinks, is polarised, and has its attraction for oxygen greatly heightened, and he offers the following representation of this phenomenon, "with an apology for the purely speculative character of the explanation." "The gaseous molecule of hydrogen being assumed to be an association of two atoms, a hydride of hydrogen, it would follow that it is the attraction of platinum for the negative, or 'chlorylous' atom of the hydrogen molecule, which attaches the latter to the metal. The tendency, imperfectly satisfied, is to the formation of a hydride of platinum. The hydrogen molecule is accordingly polarised, *orienté*, with its positive or 'basylous' side turned outwards, and having its affinity for oxygen greatly enlivened. It is true that the two atoms of a molecule of hydrogen are considered to be inseparable; but this may not be inconsistent with the replacement of such hydrogen atoms as are withdrawn, on combining with oxygen, by other hydrogen atoms from the adjoining molecules. It is only necessary to suppose that a pair of contiguous hydrogen molecules act together upon a single molecule of the external oxygen. They would form water, and still leave a pair of atoms, or a single molecule of hydrogen, attached to the platinum."

The capacity of palladium to absorb and retain hydrogen is greatly modified by its condition. Pulverulent spongy palladium takes up 655 volumes of hydrogen; when precipitated from a dilute solution of

the chloride on to platinum by the action of a voltaic battery, it forms brilliant laminæ which, when detached and gently heated in hydrogen, absorb upwards of 980 volumes of the gas, approximating, although not very closely, to the ratio H : Pd. "But," says Graham, "the idea of definite chemical combination is opposed by various considerations. No visible change is occasioned to the metallic palladium by its association with the hydrogen. Hydrides of certain metals are known as the hydride of copper (Wurtz), and the hydride of iron (Wanklyn); but they are brown pulverulent substances with no metallic characters."

Graham is inclined to the belief that the passage of hydrogen through a plate of metal is always preceded by the condensation or occlusion of the gas. "But," he adds, "it must be admitted that the rapidity of penetration is not in proportion to the volume of gas occluded; otherwise palladium would be much more permeable at a low than at a high temperature." Experiments show that the velocity of penetration increases in a rapid ratio with the temperature. The rapid dissemination of hydrogen through a soft colloid metal like palladium, or platinum at a high temperature, has a certain analogy to the process of liquid diffusion, the rate of which is greatly augmented by heat. The liquid diffusion of salt in water is six times as rapid at 100° as at 0°. "If," says Graham, "the diffusion of liquid hydrogen increases with temperature in an equal ratio, it must become a very rapid movement at a red heat. Although the quantity absorbed may be reduced (or the channel narrowed), the flow of liquid may thus be increased in velocity. The whole phenomena appear to be consistent with the solution of liquid hydrogen in the colloid metal."

There is no question that the discovery of the remarkable behaviour of palladium with respect to hydrogen greatly strengthened Graham's conviction of the validity of the hypothesis by which he sought to explain the mode of passage of gases through metals and through colloid septa, properly so called. Indeed it may be doubted whether, in the absence of this discovery, he would have continued to regard a metal like platinum as belonging to the same category, as regards this property, as india-rubber. The fact that the "penetrativeness" is most marked at high temperatures, and that practically the only gases which exhibit it are hydrogen and carbonic oxide (which are amongst the most difficult of liquefaction), are very formidable, if not insuperable objections to the liquefaction hypothesis.

In his last paper, published a few months before his death, Graham is still concerned with the relation of hydrogen to palladium. The so-called "metallic" attributes of hydrogen furnish him with what he considers may be a clue to the real nature of the combination between the hydrogen and the palladium. "It has often been maintained," he says, "on chemical grounds that hydrogen gas is the vapour of a highly volatile metal. The idea forces itself upon the mind that palladium, with its occluded hydrogen, is simply an alloy of this volatile metal, in which the volatility of the one element is restrained by its union with the other, and which owes its metallic aspect equally to both constituents."

He then seeks to determine the characters of what, on the assumption of its metallic character, he names *Hydrogenium*.

Palladium charged with hydrogen increases in bulk

to the extent of more than 4 per cent for a charge of 900 vols., and hence its specific gravity is lowered. The exact specific gravity of the "alloy" cannot be accurately determined in the ordinary way, as it continues to give off gas in minute bubbles when immersed in a liquid. By measuring the increase in length of a piece of palladium wire charged with hydrogen, of which the amount is subsequently ascertained by heating the wire *in vacuo*, some idea of the specific gravity of the hydrogenium may be obtained on the assumption "that the two metals do not contract nor expand, but remain of their proper volume on uniting." From a number of such measurements the mean density of hydrogenium was found to be 1·95, which is considerably greater than that of metallic magnesium, viz., 1·74. The expansion experienced by the charged palladium is, as Graham says, enormous if viewed as a change of bulk in the metal only, due to any conceivable physical force, amounting as it does to sixteen times the dilatation of palladium when heated from 0° to 100°.

Palladium behaves in a very remarkable manner when the hydrogen is discharged from it: *the wire contracts to an extent as much below the original length as when charged it expands beyond it.* There is, however, no alteration in the specific gravity of the metal. "The result is the converse of extension by wire-drawing. The retraction of the wire is possibly due to an effect of wire-drawing in leaving the particles of metal in a state of unequal tension, a tension which is excessive in the direction of the length of the wire. The metallic particles would seem to become mobile, and to right themselves in proportion as the hydrogen escapes; and the wire contracts in length, expanding,

as appears by its final density, in other directions at the same time."

This retraction could be effected either by heating the charged wire or by making it the positive electrode, and on charging and discharging the same wire repeatedly the retraction continued and seemed to be interminable. The metal, however, under these circumstances gradually lost much of its power to absorb hydrogen, indicating apparently some molecular or mechanical change in its nature. Indeed it was found, after a number of repetitions of the experiment, that the wire became fissured longitudinally, acquired a "thready" structure, and was much disintegrated.

The tenacity of the "alloy" is considerably less than pure palladium, in the ratio of 81 to 100. Its electric conductivity is also about 25 per cent lower. Palladium was placed by Faraday at the head of the paramagnetic elements; on charging the metal with hydrogen it becomes distinctly magnetic. Hydrogenium, therefore, would appear to take its place among the strictly magnetic metals, *i.e.* with iron, nickel, cobalt, chromium, and manganese.

Graham's most important contribution to pure chemistry was his classical memoir "On the Arseniates, Phosphates, and Modifications of Phosphoric Acid." It was communicated to the Royal Society by his predecessor in the chair of chemistry at University College, Edward Turner, and is published in the *Philosophical Transactions* for 1833. This paper, which exhibits Graham at his best, exercised an immediate and profound influence on chemical theory. It established, in the first place, the existence of what we now know as the three modifications of phosphoric acid—ortho-, pyro-, and metaphosphoric acid. Ortho- or ordinary

phosphoric acid was known to Boyle, and its widespread diffusion in nature was pointed out by Gahn and Scheele. Pyrophosphoric acid was discovered by Clark, a fellow townsman and contemporary of Graham's, whose name, perhaps, is best known in connection with the lime-process of "softening" water. The existence of the metaphosphoric acid is demonstrated for the first time by Graham in this memoir. Graham's work is memorable inasmuch as it definitely determined what is termed the *basicity* of the various modifications of phosphoric acid—that is, their power of combining with bases to form salts. Graham pointed out that the facts he indicated are most easily explained on the hypothesis that ordinary or orthophosphoric acid is characterised—to use his own words—by "a disposition to form salts which contain three atoms of base to the double atom of acid. Of these salts the most remarkable is the yellow subphosphate of silver [triargentic phosphate, $Ag_3\ PO_4$], which the soluble phosphates precipitate when added to nitrate of silver. This acid does not affect albumen; and the other modifications pass directly into the condition of this acid on keeping their aqueous solutions for some days, and more rapidly on boiling these solutions, or upon fusing the other modifications or their salts with at least three proportions of fixed base.

"Pyrophosphoric acid, or the acid which exists in the fused phosphate of soda, is remarkably disposed to form salts having two atoms base, which is the constitution of the white pyrophosphate of silver, formed on testing the pyrophosphate of soda with a salt of silver. Such salts of the preceding acid as contain no more than two atoms of fixed base pass into pyrophosphates when heated to redness. The acid under

consideration, when free, does not disturb albumen, nor produce a precipitate in muriate of barytes.

"The metaphosphoric acid is disposed to form salts, which contain one atom of base to the double atom of acid. The other modifications pass into metaphosphoric acid when heated to redness *per se*, or when heated to redness in contact with no more than one atomic proportion of certain fixed bases, such as soda. This acid, when free, occasions precipitates in solutions of the salts of barytes and of most of the other earths and metallic oxides, and forms an insoluble compound with albumen." . . . "Now it is a matter of certainty that if we take one combining proportion of any modification of phosphoric acid, and fuse it with soda or its carbonate, we shall form a metaphosphate, a pyrophosphate, or a phosphate [orthophosphate] according as we employ one, two, or three proportions of base. The acid, when separated from the base, will possess, and retain for some time, the characters of its peculiar modification. . . . I suspect that the modifications of phosphoric acid, when in what we would call a free state, are still in combination with their usual proportion of base, and that that base is water. Thus the three modifications of phosphoric acid may be composed as follows :—

Phosphoric Acid, $H_3\dot{P}$

Pyrophosphoric Acid, $H_2\dot{P}$

Metaphosphoric Acid, $H\dot{P}$"

Graham here uses the notation of Berzelius, in which each dot denotes an atom of oxygen, and writes the formulæ as binary or dualistic, in accordance with the views of the Swedish chemist. When translated into

the language of modern theory, the student will at once recognise that these facts are the common property of the text-books; but in spite of the condensation of statement which almost invariably awaits the original description of a discovery, in no text-book of to-day are they more concisely or clearly given than in Graham's own words.

The peculiar part apparently played by water in the constitution of many salts, to which Graham's attention was forcibly drawn by his investigation of the phosphates, was still further elucidated by him in a series of memoirs which appeared at various times between 1834 and 1843. Nothing, in fact, is more characteristic of Graham's work than the mode in which it appears to group itself round certain fundamental conceptions. He seems to fasten, as it were, in the outset, upon some dominant idea, which he follows to the furthest limits that are possible to him. It would not be difficult to classify the forty and odd papers with which he has enriched chemical literature, in accordance with the half-dozen leading ideas which appear to have inspired them. Thus the main principle which constitutes the basis of the most original, and perhaps the most important section of his labours, was the conception of molecular motion—a conception at the bottom, not only of his work on the diffusion and transpiration of gases, but also of that on the diffusion of saline solutions, and the transpiration or viscosity of liquids. So, too, the whole of his work on salts, embodied in about a dozen memoirs, is dominated by the central idea of the relations of water to the constitution and chemical nature of this class of substances.

Graham communicated a short paper to the meeting of the British Association in 1834, which indicates that

he was at that time occupied with an investigation, of which the results were first fully made known in a memoir entitled "Water as a Constituent of Salts," published in the *Transactions* of the Royal Society of Edinburgh for 1836. In this paper he points out that the water which is associated with the salts generically known as the magnesian sulphates, or the vitriols, plays two distinct parts. Thus, in ordinary zinc sulphate or white vitriol, of the seven molecules of water which the salt contains, one molecule is essential to, or bound up with, the constitution of the salt, whereas the remaining six molecules appear to be concerned only with its crystalline form. The six molecules of water of crystallisation are expelled when the salt is heated to 100°, or when it is placed over oil of vitriol *in vacuo* at the ordinary temperature, whilst the one molecule of water—the *saline* water, or "water of constitution," as it may be termed—is retained up to 240°. This one molecule may, however, be replaced by potassium sulphate, or with more difficulty by sodium sulphate, and a double salt formed of the type $ZnSO_4 . M_2SO_4 . 6H_2O$, where M is the alkali metal. The six molecules of water play apparently the same part as in the white vitriol, although they require a somewhat higher temperature for their expulsion than in the case of that salt.

Similar results were obtained with copper sulphate or blue vitriol. This salt usually crystallises with five molecules of water, four of which are expelled at the temperature of boiling water, whilst the fifth is retained up to about 240°, when the residue becomes white. This last molecule may be replaced by potassium sulphate, when a double salt, $CuSO_4 . K_2SO_4 . 6H_2O$, is formed, which parts with the whole of its water at about 130°. It is worthy of note, however, that the

anhydrous double salt still retains its blue colour; it may, indeed, be fused at a red-heat without becoming white like the dehydrated copper sulphate. The corresponding salt, formed with sodium sulphate, behaves in a similar manner. The sulphates of magnesia, iron, manganese, nickel, and cobalt were found to exhibit a like behaviour; a certain proportion of the water in these salts—either four or six molecules, depending on the nature of the salt—is expelled at 100°, or at ordinary temperatures *in vacuo*, whilst one molecule is in all cases retained up to at least 200°. This saline or constitutional water is in most cases replaceable by an alkaline sulphate.

The behaviour of calcium sulphate or selenite is peculiar. This salt is associated with two molecules of water, which are not expelled at 100°; at about 130° the substance loses three-fourths of its water, and if care has been taken not to exceed this temperature, it will recombine with water to form the original salt. If the sulphate is heated to above 180°, it refuses to slake or rehydrate, as already observed by Lavoisier, and is now technically known as "burnt stucco." Although double salts with alkaline sulphates are known, calcium sulphate shows much less disposition to form such salts than the vitriols—a fact which Graham is disposed to refer to the feeble affinity which calcium sulphate manifests for "saline" water. Graham's mental attitude, with respect to the part played by water in salts, is significantly illustrated in the little note he contributed in the same year to the *Philosophical Magazine*, "On the Water of Crystallisation of Soda-Alum." This salt was supposed, prior to Graham's more accurate determination, to crystallise with twenty-six molecules of water, although it is isomorphous with potash-alum, which contains only

twenty-four molecules. If the soda salt contains two molecules more of water than the potash-salt, the conclusion which follows is, he thinks, not that soda and potash are isomorphous bodies, but that soda *plus* two molecules of water is isomorphous with potash, as ammonia *plus* one molecule of water is isomorphous with the same body. Graham is constrained to admit "that the last analogy is superficial, and likely to prove illusory." Illusory it certainly proved to be; nevertheless it led him to correct the error into which his predecessors had fallen as to the amount of water contained in soda-alum; this, as in all the true alums, was found to be twenty-four molecules.

Graham was now convinced that what, in the language of the dualistic theory, were termed the hydrated acids, were constitutionally analogous to salts. The sulphate of water, SO_3+HO, according to him, is constituted like the sulphate of magnesia, SO_3+MgO. In a memoir entitled "Inquiries respecting the Constitution of Salts, of Oxalates, Nitrates, Phosphates, Sulphates, and Chlorides," published in the *Philosophical Transactions* for 1837, he seeks to show how this generalisation may be extended. There was nothing essentially novel in the fundamental conception; Davy and Dulong had independently arrived at the same conclusion, and had stated it in language in closer accord with that of modern theory. It is, however, interesting to trace the successive steps by which Graham was led to his conviction. His reasoning was based entirely on his own experimental work, and his conclusions were reached, unlike those of his predecessors, more by observation than by analogy. The paper is noteworthy also as containing a description of certain double salts up to that time unknown, as, for example,

the beautiful grass-green ferric-potassium oxalate which is familiar to every photographer who makes use of the ordinary platinotype process. It presents us with new views as to the constitution of the oxalates, many of which are here analysed for the first time, and as to the nature of what in Graham's period were known as sub-salts, but which, in accordance with the terminology with which he has familiarised us, are to-day classed as basic salts. The idea of basicity as a primary attribute of an acid is further worked out by Graham in his suggested nomenclature for the phosphates, which he constructed in order to get rid of the trivial names pyrophosphates, metaphosphates, and common phosphates, which he thinks have tended to keep up an erroneous impression that the phosphoric acid is of a different nature in these classes of salts, or is modified in some unknown way. Metaphosphoric acid was henceforth to be known as *monobasic phosphate of water*, pyrophosphoric acid as *bibasic phosphate of water*, and ordinary phosphoric acid as *tribasic phosphate of water*. Common sodium phosphate was termed *tribasic phosphate of soda and water*; microcosmic salt was *tribasic phosphate of soda, ammonia, and water*; whilst magnesium ammonium phosphate was *tribasic phosphate of magnesia and ammonia*. This nomenclature was very generally adopted in England, mainly as the result of Graham's teaching at University College, and has only recently been supplanted by a more systematic and more comprehensive terminology.

Graham found experimental support for his views concerning the essentially different parts played by the water in the vitriols in the investigations of Andrews on the heat evolved in combination. If, he argues, the

saline water in magnesium sulphate is replaceable, say, by potassium sulphate, it follows that the water and the potassium sulphate may be looked upon as equivalent in the construction of the two salts; and hence he concludes the substitution of the salt for the water should occur without evolution of heat. This, indeed, Andrews found to be the case, and Graham, on repeating the experiments, obtained the same result. He was thus led to believe that such thermal measurements might throw considerable light upon the general question, and accordingly, in 1842, he made an extensive series of measurements on the heat disengaged in combinations, accounts of which are to be found in the first two volumes of the *Memoirs of the Chemical Society*. Although the papers contain the results of a large number of observations, Graham was unable to deduce from them any important or far-reaching theoretical conclusions. His method of experiment, indeed, was hardly susceptible of the degree of accuracy needed in measurements of this character, and the results are, in many cases, undoubtedly affected by considerable errors, as is evident on comparing them with the subsequent work of Thomsen, Pfaundler, and others. At the same time certain general conclusions may be drawn from the work, which are not without interest. Equivalent quantities of the vitriols, containing seven molecules of water, appear to absorb nearly the same amount of heat on solution in water, and the same is true of the double sulphates which the vitriols form with the alkaline sulphates. On the other hand, the quantity of heat disengaged in the complete hydration of the anhydrous sulphates differs not only with the amount of water with which these substances may be assumed to combine, but also seems to vary with the

particular sulphate. As regards the heat produced by the combination of the first molecule of water—the basic or constitutional water—in the magnesian sulphates, it was found that the sulphates of water, of copper, and of manganese evolved substantially the same amount, whereas quite different quantities were disengaged in the case of the sulphates of magnesia and of zinc. In all cases the amount of heat disengaged by the union of the first molecule of water is much larger than that evolved by the combination of any subsequent molecule, which would seem to indicate an essential difference in the relation of this first molecule from that of the others.

Although Graham's intellectual powers were thus largely spent on questions of chemical philosophy, he was by no means lacking in sympathy with other departments of chemical science. From time to time he was consulted by the Government and by public bodies on matters altogether outside his special province, and with which, it might be supposed, he could have no possible concern. However uncongenial such work might seem to be, Graham's knowledge and skill were always freely given in the public interest, whether it was on the question of protecting the revenue in the matter of methylated alcohol, or protecting the consumer in that of adulterated coffee. Questions of technology, particularly when they involved the application of chemical principles hitherto unused in the arts, had always a special attraction for him, and many chemical manufacturers of the last generation were indebted to Graham for valuable suggestions and advice. As instances of his interest in applied chemistry may be quoted his papers on the "Preparation of Chlorate of Potash," and "On the Useful Applications of the

Refuse-lime of Gasworks," to be found amongst the early memoirs of the Chemical Society.

Graham served science for more than forty years. Every distinction which its votaries could render was accorded to him, and the most learned societies of the world counted him among their members. In 1833 he received the Keith Medal from the Royal Society of Edinburgh for his great work on the "Laws of Gaseous Diffusion." In 1837 the Royal Society of London awarded him a Royal Medal for his memoir on the "Constitution of Salts"; and the same Society granted him a second Royal Medal in 1850 for his work on the "Molecular Movements of Gases." In 1862 he received the Copley Medal, the highest distinction of the kind the Society can bestow. It was at one time proposed that he should allow himself to be nominated for the Presidentship, but fearing that the office would interfere with his duties at the Mint, he declined the proffered honour.

On certain Modern Developments of Graham's Ideas concerning the Constitution of Matter.

The Third Triennial "Graham" Lecture, delivered before the Philosophical Society of Glasgow in Anderson's College, 16th March 1887.

There is a certain fitness in our selecting this place to do honour to-night to the memory of Thomas Graham. For it was in the chemical laboratory of this institution that Graham carried out, upwards of half a century ago, the experimental investigations which culminated in his memorable discovery of the law connecting the rate of movement of a gas with its density. This law,

combined with that of Boyle, which connects the volume of a gas with the pressure to which it is subjected, and with the law of Dalton, which expresses the relations of the volumes of gases to heat, has done more to give precision to our knowledge of the constitution of matter than all the speculations of twenty centuries of schoolmen.

Graham was made Professor of Chemistry in the Andersonian Institution in 1830, and it was from here that he gave to the world his classical paper "On the Law of the Diffusion of Gases," read before the Royal Society of Edinburgh 19th December 1831. I am fully conscious that my only claim to be regarded as worthy to pronounce this eulogium of Graham arises from the circumstance that I also have had the good fortune to hold the Lectureship of Chemistry in this place; and with forerunners like Birkbeck, Gregory, and Graham, I may well be proud of an honourable and distinguished ancestry. This association with the Andersonian Institution naturally quickened my interest in Graham and his works, and my frequent opportunities of conversation with the late Dr. James Young, of Kelly, who for so many years was its President, and who was, as we all know, also one of Graham's "discoveries," and for a long time, both here and in London, one of his most trusted assistants, enabled me to learn much of Graham's personal character and mode of work. On the occasion of the gift of Brodie's fine statue of Graham to the city by Dr. Young, it fell to my lot to prepare the short biographical notice of my distinguished predecessor, which, with other papers relating to the matter, is, I understand, deposited in the archives of your Corporation. And I may be pardoned, perhaps, for recalling with what mingled feelings of pride and trepidation I set myself to the execution of that task.

In the preface to the admirable reprint of Graham's papers, which we also owe to the filial piety of Dr. James Young, the late Dr. Angus Smith has indicated in precise and even luminous language Graham's position in that chain of thinkers which includes Leucippus, Lucretius, Newton, and Dalton. Indeed, of all Angus Smith's papers with which I am acquainted, there is none, to my thinking, more charming than this little introductory essay of a dozen octavo pages, in which, with unwonted perspicacity, he has defined Graham's place in the history of speculative philosophy. In this paper Angus Smith has crystallised out, as it were, the thoughts of a lifetime of literary research and meditation. Probably no man—certainly no contemporary of Graham's—was better fitted by knowledge and by sympathy to form a sound critical estimate of such a position than the biographer of John Dalton. Angus Smith's mind was steeped in the old Hellenic philosophy. To him Kapila was more than a name, and the atomic systems of India matters of more than conjecture or of passing interest. Indeed there was much in Smith's intellectual nature to make such inquiries congenial to him. With all his leaning towards objective science, he had a Highlander's love of the mystical, and a Lowlander's passion for metaphysics. And yet nothing is more admirable than the manner in which in this essay these qualities and this wealth of learning are subordinated and held in check; and nothing is more striking than the way in which, in a few graphic strokes, done with a master hand, lightly yet firmly, with a consciousness of power and a sense of restraint, Graham's place in the evolution of the atomic philosophy is set forth.

It is here claimed for Graham that he was a true descendant of the early Greeks, and that to him be-

longed as of right the mantle of Leucippus. Atoms and Eternal Motion were as much fixed articles of his creed as they were of that of Heraclitus. But with no one of the older Greeks was Graham's thought more in harmony than with that of Leucippus. He, with his wider knowledge of the so-called "elemental" forms of matter, and of the persistency with which the specific properties which we associate with our "Elements" are retained, could yet share with the old Greek his conceptions of the essential oneness of matter. It was with Graham, as Smith says of Leucippus, that "the action of the atom as one substance, taking various forms by combinations unlimited, was enough to account for all the phenomena of the world. By separation and union, with constant motion, all things could be done."

In one respect Graham's position as an atomist is unique. No man before him had dedicated his life to the study of atoms and atomic motion. These fundamental ideas are intertwined to make up, so to say, the silver thread which runs through the work of forty years. They were the dominant conceptions of his life. Even in his earliest paper, published when he was just twenty-one, in which he treats of the absorption of gases by liquids, we are able to detect in the phraseology employed that his mind had been already permeated by the notion of atomic movement. That he should be familiar, even at this time, with the conception of atoms in the Daltonian sense is hardly surprising, when we remember that he had already come under the influence of Thomas Thomson, whose place in the history of science is probably that of the first great exponent of Dalton's theory of chemical combination. But the idea of motion was never with

Dalton an integral or essential part of his theory, nor, in so far as it was necessary as serving to explain the phenomena of chemical union, was it held by Thomson. And this is the more remarkable when we remember that Dalton had discovered for himself the fact of the molecular mobility of a gas, and that his first glimpses of the truth of his great law were obtained by the study of the chemical combination of gases. Graham was doubtless cognisant in a general way of the speculations of the early Greeks, but there is no evidence in any of his writings, nor has anything been preserved in the reminiscences of his friends and contemporaries, to indicate that he was knowingly influenced by them. This continuity of idea is, indeed, the most striking characteristic of Graham's labours; all his work seemed to centralise round this fundamental conception of atomic motion. "In all his work," says Smith, "we find him steadily thinking on the ultimate composition of bodies. He searches after it in following the molecules of gases when diffusing; these he watches as they flow into a vacuum or into other gases, and observes carefully as they pass through the tubes, noting the effect of weight and of composition upon them in transpiration. He follows them as they enter into liquids and pass out, and as they are absorbed or dissolved by colloid bodies, such as caoutchouc; he attentively inquires if they are absorbed by metals in a similar manner, and finds the remotest analogies, which, by their boldness, compel one to stop reading and to think if they be really possible. He follows gases at last into metallic combination, and the lightest of them all he makes into a compound with one of the heavier metals, chasing it finally through various lurking-places until he brings it into an alloy

and the form of a medal, and puts upon it the stamp of the Mint. Indeed, he is scarcely satisfied even with this, and he finds in bodies from stellar spaces—in meteoric iron—the same metallic hydrogenium, which he draws out from its long prison in the form of a gas. . . . If we examine his work on Salts and on Solutions we have a similar train of thought. One might have slighted the importance which he attached to the water of salts, and the temperature at which it was reduced, but in his hands it was a revelation of some of the most mysterious internal phenomena of these bodies.

"A chemist must take great pleasure in following Graham when he seeks the laws of the diffusion of liquids, and traces their connections, especially when they lead to such results as he expressed by dialysis, a process founded on a new classification of substances, and promising still the most valuable truths. We see in the inquiry how Graham thought on the internal constitution of bodies, by examining the motion of the parts, and from the most unpromising and hopeless masses under the chemist's hands—amorphous precipitates of alumina or of albumen—brought out analogies which connected them with the most interesting phenomena of organic life. Never has a less brilliant-looking series of experiments been made by a chemist, whilst few have been so brilliant in their results, or promise more to the inquirer who follows into the wide region opened."

In a short paper entitled "Speculative Ideas respecting the Constitution of Matter" originally published in the *Proceedings* of the Royal Society for 1863, Graham has left us his Confession of Faith upon the subject to which he had devoted the whole of a

thoughtful life. He conceives that the various kinds of matter, now recognised as different elementary substances, may possess one and the same ultimate or atomic molecule existing in different conditions of movement. Graham traces the harmony of this hypothesis of the essential unity of matter with the equal action of gravity upon all bodies. He recognises that the numerous and varying properties of the solid and liquid, no less than the few grand and simple features of the gas, may all be dependent upon atomic and molecular mobility. Let us imagine, he says, one kind of substance only to exist—ponderable matter; and further that matter is divisible into ultimate atoms, uniform in size and weight. We shall have one substance and a common atom. With the atom at rest the uniformity of matter would be perfect. But the atom possesses always more or less motion, due, it must be assumed, to a primordial impulse. This motion gives rise to volume. The more rapid the movement the greater the space occupied by the atom, somewhat as the orbit of a planet widens with the degree of projectile velocity. Matter is thus made to differ only in being lighter or denser matter. The specific motion of an atom being inalienable, light matter is no longer convertible into heavy matter. In short, matter of different density forms different substances—different inconvertible elements, as they have been considered.

It should be said that Graham uses the terms atom and molecule in a wider sense than that which the limitations of modern chemistry have imposed upon them, and that he is referring to a lower order of molecules or atoms than those which more immediately relate to gaseous volume. The combining atoms of

which he conceives the existence are not the molecules whose movement is sensibly affected by heat, with gaseous expansion as the result.

According to Graham the gaseous molecule must itself be viewed as composed of a group or system of the inferior atoms, following as a unit laws similar to those which regulate its constituent atoms. He is, in fact, applying to the lower order of atoms ideas suggested by the gaseous molecule, just as views derived from the solar system are extended to the subordinate system of a planet and its satellites. We cannot as yet fix any limit to this process of molecular division. To Graham the gaseous molecule is a reproduction of the inferior atom on a higher scale. The diffusive molecules, the molecules or systems which are affected by heat, are to be supposed uniform in weight, but to vary in velocity of movement, in correspondence with their constituent atoms. Hence, the molecular volumes of different elementary substances have the same relation to each other as the subordinate atomic volumes of the same substances.

On this basis Graham builds up a conception of chemical combination. He points out, in the first place, that these more and less mobile, or light and heavy, forms of matter have a singular relation connected with equality of volume. Equal volumes of two of them can coalesce together, unite their movement, and form a new atomic group, retaining the whole, the half, or some simple proportion of the original movement and consequent volume. Chemical combination thus becomes directly an affair of volume, and is only indirectly connected with weight. Combining weights are different because the densities, atomic and molecular, are different. The volume of combination is uniform,

but the fluids measured vary in density. This fixed combining measure—Graham's *metron* of simple substances—weighs 1 for hydrogen, 16 for oxygen, and so on with the other "Elements."

Graham, however, points out that the hypothesis admits of another expression. Just as in the theory of light we have had the alternative hypotheses of emission and undulation, so in molecular mobility the motion may be assumed to reside either in separate atoms and molecules, or in a fluid medium caused to undulate. A special rate of vibration or pulsation originally imparted to a portion of the fluid medium enlivens that portion of matter with an individual existence, and constitutes it a distinct element or substance.

The idea of the essential unity of matter finds its analogy, to Graham's thinking, in the continuity of the so-called physical states of matter. He clearly perceived that there is no real incompatibility in the different states of gas, liquid, and solid. These physical conditions are, indeed, often found together in the same substance. The liquid and the solid conditions supervene, as Graham puts it, upon the gaseous condition, rather than supersede it. They do not appear as the extinction or suppression of the gaseous condition, but as something *superadded* to that condition. Graham conceives that the three conditions (or constitutions) probably always co-exist in every liquid or solid substance, but one predominates over the others, just as the colloidal condition or constitution which intervenes between the liquid and crystalline states extends into both, and probably affects all kinds of solid and liquid matter in a greater or less degree. Hence, according to Graham, the predominance of a

certain physical state in a substance appears to be a distinction analogous to those distinctions in natural history which are produced by unequal development. Liquefaction or solidification does not involve the suppression of the atomic or molecular movement, but only the restriction of its range.

Such, then, are Graham's ideas, formulated in 1863, respecting the probable constitution of matter. I have purposely stated them in great detail, and, for the most part, in Graham's own words. The paper is very short, but it has evidently been put together with great care, and it is impossible not to be struck with the evidence it affords of Graham's insight, his grasp of principles, and power of co-ordination. Consider, for example, what he says respecting the continuity of the so-called physical states of matter, and bear in mind upon what an extremely small experimental basis it rested at that time. The observations of Cagniard de la Tour were almost forgotten, or at all events their significance was not understood. The classical work of Andrews was not yet published. And yet this work, combined with that of a dozen experimentalists in France, Russia, and Germany, has only served to confirm and expand Graham's fundamental conception. The whole paper shows Graham in a very different light from that in which the student of to-day might be apt to regard him. The greater number of his memoirs are mainly the records of measurements, but Graham was not a great measurer in the sense in which we apply that term to such men as Regnault, Magnus, or Bunsen. Very little of his work was done by his own hands, and it must be confessed that the earlier experimental portion was occasionally entrusted to apparently inexperienced assistants. Graham had, however, the

Förscherblick which characterises the true investigator, and he possessed a really marvellous faculty of sifting out the small grain of fact which often lay hidden beneath a mass of imperfect observation. And yet he was in no hurry to theorise. He patiently added fact to fact, repeating and verifying his observations long after he had got an inkling of the truth towards which they were tending. He laboured like Faraday, *ohne Hast, ohne Rast*, and his work is a monument of patient, concentrated thought, and of a singleness of purpose that never swerved.

"Experimentarian philosophers" of Graham's type (to use a phrase which Hobbes of Malmesbury once flung at the progenitors of the Royal Society) have very similar intellectual tendencies. One is insensibly led to compare Graham with the greatest of our English atomists, John Dalton. If you will turn to Dr. Henry's *Life of Dalton*, and read the careful analysis of Dalton's mental characteristics, made by one who knew him well, and who had studied him thoroughly, you will find that practically all that is there stated is equally applicable to Graham. Both men were pre-eminently endowed with the faculty of contemplating abstract relations of space and number, and each began his researches with the expectation that all empirical phenomena were to be brought under the control of mathematical laws. Thus Dalton strove to prove that the changes produced in the gaseous and liquid states of matter vary as the square, cube, or some other simple function of the temperature. Graham, in like manner, sought to show that the movement of his diffusive molecules, whether in liquids or in gases, was related to some equally simple function of their mass. Henry says of Dalton that "his inmost mental nature and all its outward

manifestations were, in the language of the German metaphysicians, emphatically subjective. Thus in special or objective chemistry he has left absolutely no sign of his presence; no great monograph on an individual body and its compounds; no memorable analysis of a substance deemed simple, into yet simpler elements; no new element—no Neptune—added to the domain of chemistry." Every word of these sentences could be applied with equal truth to Graham. The tendencies of both men were essentially introspective. Each was capable of the most patient and concentrated thought, and of steady, prolonged attention, wholly abstracted from external objects and events. I have heard the late Dr. Young narrate the most extraordinary instances of Graham's power of mental abstraction. Dalton said of himself that "If I have succeeded better than many who surround me, it has been chiefly, nay, I may say, almost solely from unwearied assiduity. It is not so much from any superior genius that one man possesses over another, but more from attention to study and perseverance in the objects before them, that some men rise to greater eminence than others."

It seems like a contradiction in terms, when we reflect for a moment upon the characteristic features and tendency of his work, to say that Graham, like Dalton, was utterly devoid of the quality we call imagination. Henry says of Dalton that imagination had absolutely no part in his discoveries, except, perhaps, as enabling him to gaze in mental vision upon the ultimate atoms of matter, and as shaping forth those pictorial representations of unseen things by which his earliest as well as his latest philosophical speculations were illustrated. Graham would not even allow his fancy that amount of play. Even in the

speculative essay from which I have quoted so largely, it seems as if every word had been weighed, and every sentence put together with slow, laborious thought. This passionless aspect of his work seems to have greatly impressed Angus Smith, himself a man of lively sympathy and of quick susceptibility. "His works," says Smith, "are full of care, but not of joy."

A quarter of a century has elapsed since Graham formulated his conceptions concerning the constitution of matter. I wish now to indicate, as briefly as may be, how these conceptions have developed during these five-and-twenty years.

The idea of the essential unity of matter has a singular fascination for the human mind. It may be that it has its germ in the persistency with which every mind, even that of a child, seeks to get at first principles. The most superficial reader of the history of intellectual evolution cannot fail to perceive how greatly it has modified and directed the development of scientific thought. The whole course of chemistry, for example, has been controlled by this fundamental conception. The unreflecting student of to-day may smile at the notion of the transmutation of the metals which held such sway over the minds of the early alchemists, but the men who followed this *ignis fatuus* with weary, faltering steps, and who frequently sank under the burden of disappointed hope, realising that to them it was not given to know the light, felt that this idea rested on a rational basis. They, like ourselves, could give a reason for the faith that was in them; and yet no article of scientific doctrine has, in these later times, suffered greater vicissitudes. Men's ideas concerning the essential unity of things must have received a rude shock when it was found that

such a thing as water was not only complex, but was made of bodies strangely contrasted in properties; that the air was still less simple in composition; and that, as it appeared, almost every form of earth could, by torture, be made to give up some dissimilar thing. The brilliant discoveries of Davy, which made the early years of this century memorable in the history of science, seemed to open out a vista to which there was no conceivable ending. The order of things was not towards simplification, but rather towards complexity; and yet Davy himself seemed unable or unwilling to push his way along the path of which the world regarded him as the pioneer. It may be that he was unable to shake himself free from the domination of the schoolmen, or that he unconsciously felt the truth of the principle to which his own discoveries seemed opposed. It is difficult otherwise to account for the tardiness with which he accepted the hypothesis of Dalton; even to the last the Daltonian atom had nothing distinctive to him beyond its combining weight. Davy never wholly committed himself to a belief in the indivisibility of the atom; that indivisibility was the very essence of Dalton's creed. In arguing with a friend concerning the principle of multiple proportion, Dalton would clinch the discussion by some such statement as "thou knows IT MUST BE SO, for no man can *split an atom.*" Even Thomas Thomson, whom I have already characterised as the first great exponent of Dalton's generalisation, was swayed by conflicting beliefs until he found peace in the hypothesis of Prout and Meinecke, that the atomic weights of all the so-called elements are multiples of a common unit, a conception he sought to establish by some of the worst quantitative determinations to be found in chemical

literature. It is curious to note the bondage in which the old metaphysical quibble concerning the divisibility or indivisibility of the atom held the immediate followers of Dalton. Graham, however, never felt such trammels; to him the atom meant something which *is not* divided, not something which *cannot* be divided. With Graham, as with Lucretius, the original atom may be far down. Every philosophic thinker to-day has, I should imagine, come to be of this opinion. Not many years ago it was the fashion to maintain that Stas's great work had for ever demolished the doctrine of the primordial *ylé*, and that Roger Bacon's aphorism that "barley is a horse by possibility, and wheat is a possible man, and man is possible wheat," was henceforth an idle saying. Stas's work is a monument of experimental skill, and it has furnished us with a set of numerical ratios which are among the best determined of any physical constants. It may be that it demolished Prout's hypothesis in its original form, but it has not touched the wider question; indeed it is very doubtful whether the wider question is capable of being reached by direct experiments of the nature of those of Stas, unless the weight of the common atom is some very considerable fraction, say one-half or one-fourth of that of the hydrogen atom. Dumas has, as you know, modified Prout's hypothesis in this sense by assuming as the common divisor half the atomic weight of hydrogen, but there is no *a priori* reason why we should stop at this particular subdivision. The exact relation of Stas's work to Prout's law has, I think, been fairly stated by Professor Mallet at the conclusion of his admirable paper "On the Atomic Weight of Aluminium," in the *Philosophical Transactions* for 1880 (vol. clxxi. 1033). Stas's main result,

says Mallet, "is no doubt properly accepted if stated thus, that the differences between the individual determinations of each of sundry atomic weights which have been most carefully examined are distinctly less than their difference, or the difference of their mean from the integer which Prout's law would require. But the inference which Stas himself seems disposed to draw, and which is very commonly taken as the proper conclusion from his results, namely, that Prout's law is disproved or is not supported by the facts, appears much more open to dispute. It must be remembered that the most careful work which has been done by Stas and others, only proves by the close agreement of the results that *fortuitous* errors have been reduced within narrow limits. It does not prove that all sources of constant error have been avoided, and indeed this never can be absolutely proved, as we never can be sure that our knowledge of the substances we are dealing with is complete; of course, one distinct exception to the assumed law would disprove it, if that exception were itself fully proved, but this is not the case. As suggested by Marignac and Dumas, any one who will impartially look at the facts can hardly escape the feeling that there must be some reason for the frequent recurrence of atomic weights differing by so little from accordance with the numbers required by the supposed law." Professor Mallet in tabulating the atomic weights which may be fairly considered as determined with the greatest attainable precision, or a very near approach thereto, and without dispute as to the methods employed, points out that out of the 18 numbers so given, 10 approximate to integers within a range of variation less than one-tenth of a unit. He then proceeds to calculate the degree of probability that

this is purely accidental, as those hold who carry to the extreme the conclusions of Berzelius and Stas, and he finds that the probability in question is only equal to 1 : 1097·8, and he concludes that not only is Prout's law not as yet absolutely overturned, but that a heavy and apparently increasing weight of probability in its favour, or in favour of some modification of it, exists and demands consideration.

It would be impossible for me to attempt to traverse even so much of the whole ground of this question as has been opened up during the past fifteen or twenty years. Even if I could claim the time and your indulgence, there is hardly the necessity for such a demand on your patience. Mr. Crookes, only so recently as September last, gave an admirably complete exposition of the present state of the case in his address to the Chemical Section of the British Association at the Birmingham Meeting, and for me to go over the field again with you would be simply to plough with Mr. Crookes' heifer. Some years ago Mr. Norman Lockyer, as you doubtless know, approached the subject from another point of view, and in his recent work, *The Chemistry of the Sun*, you will find a summary of the evidence which the spectroscope has afforded us concerning the dissociation of "elementary" matter at such transcendental temperatures as we have in stars like Sirius.

Now, when we pass in review all this evidence; when we reflect upon the mode of distribution of the elements, and especially their tendency to associate in correlated groups; when we bear in mind the absolute analogy which exists in the general behaviour and mode of action of the radicles which are confessedly compound with those which are assumed to be simple;

when we have regard to the phenomena of allotropy, isomerism, and homology,—the mind insensibly appeals to the principle of continuity and refuses to believe that the seventy and odd "elemental" forms, to which our processes of analysis have reduced all the kinds of matter we see around us, differ in essence from bodies which are known to be compound.

The connection between the properties of the "elements" and the relative weights of their atoms, as developed by Newlands, Mendeleeff, Lothar Meyer, Carnelley, and others, has served to strengthen this conviction. The discovery that the physical and chemical properties of the elements are periodic functions of their atomic weights is unquestionably the most important generalisation we have had in chemical philosophy during the last five-and-twenty years. Its bearings upon the question of the origin of the "elements" have been worked out in the Presidential Address I have already referred to. Mr. Crookes, like Mr. Lockyer before him, in seeking to apply to this question of the genesis of the "elements" the same principles of evolution which Laplace has already applied to the creation of the heavenly bodies, and which Lamarck, Darwin, and Wallace have applied to that of the organic world, is again appealing to the law of continuity. The mind which holds that nature is one harmonious whole is fain to believe that the probability that the elements have originated by chance, and are eternally self-existent, is just as remote as that the animals and plants of to-day are primordially created things. I think, in what I am now saying I may fairly claim to be reflecting the opinion on this matter of every philosophic thinker of to-day. Nay, more: you must allow that the germ which has been

kept alive for so many centuries, and which has come down to us through the brains of a succession of thinkers like Leucippus, Aristotle, Lucretius, Bacon, Newton, Dalton, and Graham, has become quickened and endowed, by the light which modern science has shed upon it from all sides, with a vitality which will persist and strengthen.

Having thus traced the development of the idea held by Graham of the essential oneness of matter, let us spend the few remaining moments in considering, in the most general way, how the science of the last twenty-five years has worked out and extended his conceptions concerning the properties of the atom and its mode of motion.

The treatment which "the few grand and simple features of the gas," to quote Graham's phrase, has received at the hands of Clausius, Clerk-Maxwell, Helmholtz, Sir William Thomson, and of a score of workers in this country and on the Continent who have been actuated by their influence, has served to dispel much of the metaphysical fog which has enshrouded the notion of the atom, and to-day we are able to reason about atoms, as physical entities, having extension and figure, and to speak of their number and dimensions and peculiarities of movement, with a confidence based on well-ascertained facts. We have, of course, not yet attained to a complete molecular theory of gases. But we know the relative masses of the molecules of various gases, and we have calculated in miles per second their average velocity. The phenomena of diffusion indicate that the molecules of one and the same gas are all equal in mass. For, as was pointed out by Clerk-Maxwell, if they were not, Graham's method of using a porous septum would

enable us to separate the molecules of smaller mass from those of greater, as they would stream through porous substances with greater velocity. We should thus be able to separate a gas, say hydrogen, into two portions, having different densities and other physical properties, different combining weights, and probably different chemical properties of other kinds. As no chemist has yet obtained specimens of hydrogen differing in this way from other specimens, we conclude that all the molecules of hydrogen are of sensibly the same mass, and not merely that their mean mass is "a statistical constant of great stability." (See Art. "Atom," *Encyclopædia Britannica*, 9th ed.) This line of argument has, it seems to me, an important bearing upon a question which has been raised by Marignac, Schützenberger, and others, and which has again been raised by Mr. Crookes in the address I have already referred to. Mr. Crookes thinks that it may well be questioned whether there is an absolute uniformity in the mass of every ultimate atom of the same chemical element, and that it is probable that our atomic weights merely represent a mean value, around which the actual atomic weights of the atoms vary within certain narrow limits, or, in other words, that the mean mass is "a statistical constant of great stability." The facts of diffusion would seem to lend no support to such a supposition.

Graham was still living when Loschmidt published what Exner calls his epoch-making paper "On the Size of the Air Molecule." Although the numerical estimate which Loschmidt deduced from the mean free path of the molecules and their volume has now only an historical interest, it has exercised a profound influence on the development of molecular physics in

demonstrating that in dealing with molecules we are dealing with masses of finite dimensions, and further, that these dimensions are by no means immeasurably small. The very manner in which Loschmidt stated his conclusions was well calculated to rivet attention. He showed that these magnitudes, small as they are, are yet comparable with those which can be reached by mechanical skill. The German optician Nobert has ruled lines on a glass plate so close together that it requires the most powerful microscope to observe the intervals between them; he has drawn, for example, as many as 4000 lines in the breadth of a millimetre—that is, about 112,000 lines to the inch. Now, if we assume with Maxwell that a cube whose side is the 4000th of a millimetre is the smallest volume observable at present, it would follow from Loschmidt's calculations that such a cube would contain from 60 to 100 millions of molecules of oxygen or nitrogen; and if we further assume that the molecules of organised bodies contain on an average 50 "elementary" atoms, it further follows that the smallest organised particle visible under the microscope contains about 2 million molecules of organic matter. And as at least half of every living organism is made up of water, we arrive at the conclusion that the smallest living being visible under the microscope does not contain more than about a million organic molecules. I could have wished, had time permitted, to dwell a little upon the intensely interesting questions which such a conclusion at once raises. In the article "Atom" in the *Encyclopædia Britannica*, from which I have quoted, you will find Clerk-Maxwell points out its relation to physiological theories, and especially to the doctrine of Pangenesis. "Molecular

science," says Maxwell, "forbids the physiologist from imagining that structural details of infinitely small dimensions can furnish an explanation of the infinite variety which exists in the properties and functions of the most minute organisms."

In the year following Graham's death Sir William Thomson still further developed the modes of molecular measurement, and from a variety of considerations, based upon the kinetic theory of gases, upon the thickness of the films of soap bubbles, and from the electrical contact between copper and zinc, he arrived at estimates which, although sensibly different from that of Loschmidt, are still commensurable with it. In a lecture at the Royal Institution, given about four years ago, he extends the lines of his argument and arrives at the conclusion that in any ordinary liquid, transparent solid, or seemingly opaque solid, the mean distance between the centres of contiguous molecules is less than the one five-millionth and greater than the one thousand-millionth of a centimetre; and in order to give us some conception of the degree of coarse-grainedness implied by this conclusion, he asks us to imagine a globe of water or glass, as large as a football, to be magnified up to the size of the earth, each constituent molecule being magnified in the same proportion. The magnified structure would be more coarse-grained than a heap of small shot, but probably less coarse-grained than a heap of footballs (*Nature*, 19th July 1883).

Here, I think, we may leave the subject, at all events for to-night. I am painfully conscious that I have left unsaid much that ought to have been said, and possibly said some things that might well have been left unsaid. But my main purpose will have been served if I have

succeeded in indicating to you Graham's position as an atomist, and in showing you how his ideas respecting the constitution of matter have germinated, and, like the seed which fell upon good ground, have borne fruit an hundredfold.

X

FRIEDRICH WÖHLER

A Lecture delivered at the Royal Institution, Albemarle Street, on Friday Evening, 15th February 1884.

It seems fitting that these walls, which have vibrated in sympathy with that brilliant eulogy of Liebig, which Professor Hofmann pronounced some nine years ago, should hear something of him whose lifelong association with Liebig has exercised an undying influence on the development of scientific thought. The names of Friedrich Wöhler and Justus Liebig will be linked together throughout all time. The work which they did in common marks an epoch in the history of chemistry. No truer indication of the singular strength and beauty of their relations could be given than is contained in a letter from Liebig to Wöhler, written on the last day of the year 1871. "I cannot let the year pass away," writes Liebig to Wöhler, "without giving thee one more sign of my existence, and again expressing my heartfelt wishes for thy welfare and the welfare of those that are dear to thee. We shall not for long be able to send each other New Years' greetings, yet, when we are dead and mouldering, the ties which have united us in life will still hold us together in the memory of men as a not too frequent example of faithful workers who, without envy or

jealousy, have zealously laboured in the same field, linked together in the closest friendship."

And yet, bound as they were in the ties of a friendship, the purity and warmth of which were but characteristic of the men, and although each influenced the other's walk and work in life to a degree which it is almost impossible to gauge, such was the strength of their individuality, and such the force of their genius that, without a doubt, either would have been a great figure in the history of science if the other had not existed.

The conditions under which minds of the highest type arise and develop have on more than one occasion engaged the attention of this audience. Although there were circumstances in Wöhler's surroundings which in early life may have influenced the bent of his mind, it is not easy to see whence sprang that passionate love of nature which was so strikingly exhibited in the man. His father, August Anton Wöhler, was formerly an equerry in the service of the Elector William II. of Hesse; he afterwards came to live at Frankfort, and became a leading citizen of that town. His wise liberality and public spirit are commemorated in the Wöhler Foundation and Wöhler School, institutions known to every Frankforter. His mother was connected by marriage with the minister of Eschersheim, a village near Frankfort, and it was in the minister's house that Friedrich Wöhler first saw the light, on 31st July 1800. Even in early youth his passion for experimenting and collecting manifested itself, to the neglect not unfrequently of the lessons of the gymnasium; indeed, it would appear that during his school career Wöhler was not characterised by either special diligence or knowledge. The bent of his mind towards natural science was

directed by Dr. Buch, a retired physician, who had devoted himself to the study of chemistry and physics; and it was in the kitchen of his patron's house that he prepared the then newly-discovered element selenium, of which an account was afterwards sent by Dr. Buch to Gilbert's *Annalen*, with Wöhler's name at the head of it. The elder Wöhler appears to have been a man of considerable artistic feeling, and under his direction the son was taught sketching, and otherwise educated in that perception of natural beauty which comes out so strikingly in his after life; and he was encouraged to make himself familiar with the literature which the genius of Schiller and Goethe has ennobled. He had, moreover, to thank his father for that love of physical exercise and passion for outdoor life which reacted so beneficially upon his development, and contributed so largely to the uniformly good health which he enjoyed to within a few days of his death. Mainly, it would seem, because his father had been there before him, Wöhler, in his twentieth year, entered the University of Marburg. It was his own and the family's wish that he should study medicine, and he accordingly put his name down for the lectures of Bünger on Anatomy, Gerling on Physics and Mathematics, and Wenderoth on Botany. He found time also to attend Ullmann's classes on Mineralogy; and although he declined to hear Wurzer's lectures on Chemistry, he by no means neglected that science. He transformed his living-room into a laboratory, and to the great, and perhaps not undeserved, disgust of his landlady, occupied himself with the preparation and study of the properties of prussic acid, thiocyanic acid, and other cyanogen compounds. He discovered at that time, without knowing that Sir Humphry Davy had anticipated him, the beautifully

crystalline but intensely poisonous iodide of cyanogen; and in the little paper on cyanogen compounds which his good friend Dr. Buch communicated to Gilbert's *Annalen* for him we have the first description of the remarkable behaviour of mercuric thiocyanate on heating, which has astonished and amused us in the so-called "Pharaoh's Serpent."

Wöhler, attracted by the fame of Leopold Gmelin, left Marburg for Heidelberg. His main idea was to hear the lectures of that distinguished man, but Gmelin declared this to be unnecessary and a waste of time. Wöhler in fact never attended any systematic lectures on chemistry; he had access, however, to the old cloisters which at that time constituted the Heidelberg laboratory, and there began the work on cyanic acid which, some four or five years later, was destined to culminate in the great discovery of the synthesis of urea. His association, at this time, with Tiedemann, who was engaged in physiological chemical investigations with Gmelin, had also considerable influence in determining the direction of much of his future work, whilst its immediate effect was the publication in Tiedemann's *Zeitschrift für Physiologie* of the results of an inquiry into the transformation experienced by various substances, organic and inorganic, in their passage through the organism.

In 1823 Wöhler obtained his degree, when, on Gmelin's advice, he determined to follow his master's example, and abandon medicine for chemistry. At that time the great Swedish chemist Berzelius was at the summit of his fame; his masterly analytical skill, no less than his labours towards the development of chemical theory, had made him supreme among the chemists of Europe; and to Stockholm, therefore,

Wöhler, acting on the advice of Gmelin, determined to go. He was warmly welcomed by Berzelius, on whom his communications to Gilbert's *Annalen* had made a favourable impression, and with the offer of a place in the private laboratory of the illustrious Swede, Wöhler set out for the Scandinavian capital. Of his experiences with Berzelius his pupil has left us a delightful description. It is valuable not only as a charming character-sketch of the great teacher, but also from the side-light it throws upon the nature and disposition of Wöhler himself. It is interesting, too, as an account of the mode in which Berzelius worked and taught, and as showing how the typical laboratory of that time contrasted with the temples which have since been reared by the disciples of Hermes.

"With a beating heart," says Wöhler, "I stood before Berzelius's door and rang the bell. It was opened by a well-clad, portly, vigorous-looking man. It was Berzelius himself. . . . As he led me into his laboratory I was as in a dream, doubting if I could really be in the classical place which was the object of my aspirations. . . . I was at that time the only one in the laboratory; before me were Mitscherlich and Heinrich and Gustav Rose; after me came Magnus. The laboratory consisted of two ordinary rooms furnished in the simplest possible way; there were no furnaces or draught places; neither gas nor water service. In one of the rooms were two common deal tables; on one of these worked Berzelius, the other was intended for me. On the walls were a few cupboards for the reagents; in the middle was a mercury trough, whilst the glass-blower's lamp stood on the hearth. In addition was a sink, with an earthenware cistern and tap, standing over a wooden tub, where the despotic Anna, the cook,

had daily to clean the apparatus. In the other room were the balances, and some cupboards containing instruments; close to was a small workshop fitted with a lathe. In the neighbouring kitchen, in which Anna prepared the meals, was a small but seldom-used furnace and the never-cool sand-bath."

Wöhler's first exercises were in mineral analysis, made in order that he might become acquainted with Berzelius's special methods and manipulative procedure. At that time he prepared, among other products, some new compounds of tungsten, notably the beautifully crystallised monoxychloride, and the tungsten sodium-bronze ($Na_2W_3O_9$), which, some twenty-five years later, was introduced into the arts as a bronze powder. It was, however, with his investigation on cyanic acid that both he and Berzelius were mainly interested. To Berzelius the existence of this body was of importance from the light it seemed to him to throw upon the validity of the new chlorine theory. "I was surprised," says Wöhler, "to hear him, the hitherto steadfast upholder of the old notion, now always talk of *chlorine* instead of 'oxidised hydrochloric acid.' Once, when Anna, in cleaning some vessel, remarked that it smelt strongly of oxymuriatic acid, Berzelius said, 'Hearest thou, Anna; thou must no longer speak of oxidised muriatic acid; thou must call it *chlorine:* that is better.'" With what feelings would Davy have listened to that colloquy between the Swedish philosopher and his factotum! Chlorine was discovered by Berzelius's countryman, Scheele, but its true nature was first demonstrated in the laboratory of the Royal Institution.

A couple of months were now spent in travel with Berzelius, in company with the two Brongniarts, Alexandre the geologist and Adolphe the botanist,

during which they explored the greater portion of the geologically interesting parts of Southern Sweden and Norway, and collected rich stores of those wonderful minerals for which Scandinavia is famous. Scandinavia is no less famous for salmon and trout; and it was on his return from a fishing expedition in Norway that the travellers met with Davy, who, as readers of *Salmonia* know, handled his rod with great zest and skill. Wöhler, who as a boy had learned the story from his friend Dr. Buch of the isolation of the alkali metals by Davy, and who, aided by his little sister, whose business it was to blow the bellows, had toiled, not unsuccessfully, to make potassium in the kitchen fire, was presented to the famous chemist.

At the end of the tour Wöhler took leave of Berzelius and returned to Germany. Of his association with the great teacher Wöhler had ever the kindliest memories. Although the outcome of much of his subsequent work, or at least much of that which he did in concert with Liebig, might be said to bring him in occasional conflict with Berzelius's cherished convictions on points of chemical theory, the master and pupil remained to the end in the ties of the warmest friendship. Scarcely a month passed without an exchange of letters. Those from Berzelius were carefully preserved by Wöhler, who, after his master's death in 1848, presented them, to the extent of some hundreds, to the Swedish Academy of Sciences. We are told that in the later letters the "trauliche Du" appears in place of the more formal "Sie," and that *Totus et tantus tuus* is a not unfrequent signature.

Wöhler's gratitude and almost filial reverence are seen in the circumstance that even in the full tide of his vigour, and when time was doubly precious to him, he

continued to charge himself with the yearly translation of Berzelius's *Jahresbericht* into German. It is easy to trace the influence of Wöhler's contact with Berzelius in his after-work. To begin with, the men had much in common; their sympathies were as catholic as science itself, and they ranged at will over every department of chemical knowledge. Wöhler attacked the composition of a mineral with as much ardour as he did the preparation of an organic compound; to him the problems of physiological chemistry were not more important than the isolation of a rare earth or the perfection of some analytical method. The artificial barriers and arbitrary lines of demarcation in the science seemed to have no existence for him; indeed, it was the crowning triumph of his work to break down such barriers almost at a stroke, and to demonstrate the irrationality of attempts to draw distinctions in the absence of differences. The history of chemistry is indeed like that of the nation which has done so much to advance it; its unity to-day is as complete as that of Germany itself.

Wöhler, now back again in Germany, prepared to embark on his academic career, and on the advice of Gmelin and Tiedemann he decided to settle in Heidelberg as a *privat docent*. But to Heidelberg he was not destined to go. His work had already been gauged by Leopold von Buch, Poggendorff, and Mitscherlich, and these, without his knowledge, strongly recommended that he should be elected to the vacant teachership of chemistry in the newly founded Trade School in Berlin. Berzelius advised him to accept the post, and accordingly to Berlin Wöhler went in 1825. He was now in possession of a laboratory which he could call his own, and he had to justify that possession by the use which

he made of it. One of the problems which he at this time attacked was the isolation of *aluminium*, a metallic radicle more abundant and more widely diffused than any other of the fifty substances we are accustomed to designate as metals. He succeeded in obtaining the metal by the method which, nearly twenty years later, was worked out on a manufacturing scale by Sainte-Claire Deville. Deville caused the first bar of aluminium thus procured to be struck into medals, with the image of Napoleon III. on the one side, and the name Wöhler with the date 1827 on the other, and some time afterwards the Emperor simultaneously designated the two chemists officers of the Legion of Honour.

But of the twenty-two memoirs and papers which Poggendorff's *Annalen* exhibits as the outcome of Wöhler's activity and power of work during his six years' stay in Berlin, that on the artificial formation of urea is by far the most important. No single chemical discovery of this century has exercised so great an influence on the development of scientific thought, and the words with which Wöhler closes his account of the molecular transformation of ammonium cyanate—a body of purely inorganic origin—into urea—a substance which of all that might be named is most characteristic of the action of the so-called vital force—are full of meaning: "This unexpected result," he says, "is a remarkable fact, in so far as it presents an example of the artificial formation of an organic body, and indeed one of animal origin, out of inorganic materials." "The synthesis of urea," says Professor Hofmann in his account of Wöhler's life-work, "was an epoch-making discovery in the real sense of that word. With it was opened out a new domain of investigation, upon which the chemist instantly seized. The present generation,

which is constantly gathering such rich harvests from the territory won for it by Wöhler, can only with difficulty transport itself back to that remote period in which the creation of an organic compound within the body of a plant or an animal appeared to be conditioned in some mysterious way by the vital force, and they can hardly realise the impression which the building up of urea from its elements then made upon men's minds. And yet it cannot be said that chemists were unprepared for this discovery. Men were long ago in the habit of perceiving that bodies of mineral origin were but the types of those met with in the animal and vegetable organism—in both classes there were the same differences in states of aggregation, the same mutual transformations, the same crystalline forms, the same constancy in combining relations, the same conjunction of the elements according to the weights of their atoms or in multiples of these, in both classes the appearance of the same species of compounds. But all attempts to build up organic compounds from their elements, as this for a large number of mineral substances had already been done, had hitherto been futile. The chemists of that period had nevertheless the presentiment that even this barrier must fall, and one can conceive the feeling of joy with which the gospel of a new unified chemistry was hailed by the intellect of that time. With the revolution thus effected in the ideas of men, science was directed into new paths and unto new goals. Who does not know with what zeal these paths have been trodden, and how many of these goals have been reached!"

But if at this time Wöhler made a great discovery for the world, he also, at about the same time, made a great discovery for himself: he found Liebig. The manner in which the two men were brought together is

worth mentioning, for it would seem almost as if the hand of destiny was in it. At about the period that Wöhler was in Stockholm thinking and working on cyanic acid, Liebig was in Paris engaged with Gay Lussac in the study of the metallic compounds of fulminic acid, which obtains its not inappropriate name on account of the formidable explosive character of its salts. Liebig, with rare skill and courage, had determined the composition of that acid, and had been rewarded by the honour of a waltz with Gay Lussac, it being the habit of that distinguished philosopher, as he explained to the astonished young German doctor, to express his ecstasy on the occasion of a new discovery in the poetry of motion. But the most extraordinary result of that investigation was to show that the terribly explosive fulminic acid and the innocuous cyanic acid were of identical composition. The idea that bodies could exist of identical ultimate composition —that is, composed of the same elements united in the same proportion—and yet possess essentially different properties—in other words, be absolutely dissimilar things—was new to science. Berzelius, the great chemical lawgiver of his time, scouted the notion as absurd; to him it was impossible to conceive that identity in elementary composition should not result in identity of properties. And yet, later on, Berzelius was forced to realise the fact by Wöhler's discovery of the molecular transformation of ammonium cyanate into urea, and to coin for us the word *isomerism*, by which that fact is denoted.

It was thus, from the singular circumstance that Wöhler and Liebig were at the outset of their careers engaged upon the elucidation of the nature of two bodies of identical composition, but of dissimilar origin,

dissimilar relations, and very different properties, that they were brought into juxtaposition. They desired to know each other; they met in the house of a mutual friend at Frankfort, and henceforth the names of Liebig and Wöhler became linked together for all time.

The origin of the partnership, so fruitful in consequences for science, may be seen from the following characteristic letter :—

FRIEDRICH WÖHLER TO JUSTUS LIEBIG.

SACROW, NEAR POTSDAM, *8th June*, 1829.

DEAR PROFESSOR—The contents of your last letter to Poggendorff have been communicated to me by him, and I am glad that they afford me an opportunity of resuming the correspondence which we began last winter. It must surely be some wicked demon that again and again imperceptibly brings us into collision by means of our work, and tries to make the chemical public believe that we purposely seek these apples of discord as opponents. But I think he is not going to succeed. If you are so minded, we might, for the humour of it, undertake some chemical work together, in order that the result might be made known under our joint names. Of course, you would work in Giessen, and I in Berlin, when we are agreed upon the plan, and we could communicate with each other from time to time as to its progress. I leave the choice of subject entirely to you.

I am very glad that you have also determined the identity of pyrouric acid, and cyanic [cyanuric] acids. L. Gmelin would say : "God be thanked, there is one acid the less !" . . .—Yours,

WÖHLER.

Liebig acceded to the proposition at once, and suggested some problem on the chemical nature of nitrogen; this Wöhler found himself unable to undertake, as it involved the use of chlorine, to the action of which he was at all times extremely susceptible. On the other hand, he proposed to Liebig that they should

continue in common a research on mellitic acid, which he had himself begun. Their joint investigation on this body made its appearance in the course of the following year.

It would be quite impossible within the limits of an hour to attempt to give you anything approaching to a complete analysis of Wöhler's work. In all, he was the author of 275 memoirs and papers, and of these fifteen were published in concert with Liebig. I must therefore confine my selection from this vast amount of material to those papers which are of paramount importance by reason of the influence which they have exerted on chemical theory or on the development of the chemical arts.

Very shortly after the publication of the work on mellitic acid, Wöhler proposed to Liebig a joint investigation on cyanuric acid, in the course of which he observed the extraordinary transformation of that acid into cyanic acid, and the reconversion of the cyanic acid into cyanuric acid—one of the most remarkable instances of molecular rearrangement known to the chemist. The work progressed little for some months, owing to the demands made by Berzelius's *Jahresbericht* on Wöhler's time. "Wirf die Schreiberei zum Teufel," wrote Liebig, "und gehe in das Laboratorium, wohin Du gehörst." In due time, doubtless, that functionary carried off the writing to his master, the printer, and Wöhler went back to his laboratory, and in a few weeks the two investigators obtained the clue to the puzzle. Liebig wrote to Wöhler: "Now that I have received your experiments, the whole thing is cleared up, and with what satisfaction for us! The matter is now decided; the cyanic acid of Serullas is identical with that from urea. . . . Ich bin ganz närrisch vor Freude

dass unser Kindlein nun fehlerlos in die Welt gesetzt wird, ohne Buckel oder Klumpfuss."

It had been suggested to attack the fulminic acid again. "The fulminic acid we will allow to remain undisturbed. Like you, I have vowed to have nothing more to do with this stuff. Some time back I wanted, in connection with our work, to decompose some fulminating silver by means of ammonium sulphide; at the moment the first drop fell into the dish the mass exploded under my nose. I was thrown backwards, and was deaf for a fortnight, and became almost blind."

The work on cyanic acid appeared in Poggendorff's *Annalen* during the last month of 1830, and Wöhler was able to send the "Kindlein" "im neuen Kleide," as he says, with a New Year's greeting to his friend. Liebig had suggested fresh work, but at the moment Wöhler was in no humour to attack anything organic. The Swedish chemist Sefström had just announced the existence of a new element in the slag of certain iron ores, and this very substance had slipped through Wöhler's fingers unperceived. "I was an ass," he wrote to his friend, "not to have detected it two years ago in the lead ore from Zimapan in Mexico. I was busy with its analysis, and had found something strange in it, when I was laid up for some months in consequence of breathing hydrofluoric acid, and so the matter was allowed to rest. Meanwhile Berzelius sends me word of its discovery by Sefström in Swedish bar iron and in slag. It is very like chromium, and just as remarkable. Moreover, it is the same metal that Del Rio found in the Mexican lead ore, and called erythronium: Descotils, however, had declared this ore to be lead chromate."

Wöhler, no doubt, found a ready sympathiser in

Liebig, to whom, not many years before, a similar experience had happened. We all know the story of the young chemist whose unscientific use of the imagination cost him the discovery of the element *bromine*. Wöhler had sent some of the substance from the Zimapan ore to Stockholm, and Berzelius wrote as follows :—

JAKOB BERZELIUS TO FRIEDRICH WÖHLER.

STOCKHOLM, 22*nd January* 1831.

As to the small quantity of the body marked ? I will relate the following story : "In the far north there lived in the olden time the goddess Vanadis, as beautiful as she was gracious. One day there came a knock at her door. The goddess was in no hurry, and thought, 'They can knock again'; but no further knock came, for he who knocked had passed on. The goddess, wondering who it could be that cared so little to be let in, ran to the window and recognised the departing one. 'Ah!' said she to herself, 'it is that lazy fellow Wöhler! He richly deserves his name, since he cares so little to come in.' Some days after, some one else knocked, repeatedly and loud. The goddess opened the door herself; it was Sefström who entered, and, as a consequence, vanadium came to light." Your specimen with the ? is, in fact, vanadium oxide.

But he that has found the mode of artificially forming an organic body can well renounce the discovery of a new metal; indeed, one might have discovered ten unknown elements without as much skill as is seen in the masterly work which you and Liebig have carried out together and have just communicated to the scientific world.

In 1831 Wöhler was called from Berlin to Cassel, and for some little time he was wholly engaged in the planning and erection of his new laboratory at the Gewerbe-Schule in that town. In the spring of the following year he was again ready for a new research; and this time it was to be the most fruitful piece of

work that the two investigators jointly engaged in. It was, in fact, to be the classical research on bitter almond oil. On 16th May 1832 Wöhler wrote to Liebig: "Ich sehne mich nach einer ernsten Arbeit, sollten wir nicht die Confusion mit dem Bittermandelöl in's Reine bringen? Aber woher Material?" It must have been something akin to inspiration which led Wöhler to take up this subject; but neither he nor Liebig could have been wholly conscious of the consequences which were to follow from their work. To-day oil of bitter almonds is made artificially in Germany by the hundredweight; at that time the investigators could only obtain it in small quantities from Paris. They had indeed to thank Pelouze for the material with which they worked. Wöhler made this, his greatest research, under the cloud of a great sorrow: after barely two years of married life he lost his wife. Liebig, in the tenderest manner, brought him over to Giessen, and sought to win him from his grief and the sense of his loneliness by his company and the wholesome distraction of their joint work, done side by side.

On 30th August 1832 Wöhler wrote to Liebig from Cassel:—

"I am here back again in my darkened solitude. I do not know how I shall thank you for the affection with which you received me and kept me by you for so long. How happy was I that we could work together face to face.

"I send you with this the memoir on bitter almond oil. The writing has taken me longer than I anticipated. I want you to read through the whole with the greatest care, and to notice particularly the numbers and formulæ. What does not please you, alter at once. I have often felt that there was something not

quite right, without being able to detect what was wrong."

I shall not attempt to dwell upon the outcome of this great work. The investigation on the radicle of benzoic acid will ever remain one of the greatest achievements in the history of organic chemistry; the work was indeed epoch-making in the far-reaching nature of its consequences. It was full of facts and rich in the promise of new material—a veritable mine from which subsequent workers like Cannizzaro, Fehling, Piria, Stas, and Hlasiwetz have dug rich treasure. The immediate effect of the paper was to establish the doctrine of organic radicles by demonstrating the existence of groups of bodies which had their analogues and prototypes in inorganic chemistry. The concluding words of the memoir strike, in fact, the keynote of the whole investigation. "In once more reviewing and connecting together the relations described in this memoir," so wrote Liebig and Wöhler, "we find that they may be grouped round a common nucleus which preserves intact its nature and composition in its associations with other bodies. This stability has induced us to regard this nucleus as a kind of compound element, and to propose for it the special name of 'benzoyl.'"

A significant feature in the memoir was that each of the substances described and correlated was the type of a distinct group of bodies, some of which were known, but of which the analogies and relations were unperceived; others of these bodies were yet to be discovered, a matter of little difficulty when the modes of their origin had been indicated. The effect of this memoir on the chemical world was instantaneous. Berzelius was delighted. "The facts put forward by you," he

wrote to Wöhler and Liebig, "give rise to such considerations that they may well be regarded as the dawn of a new day in vegetal chemistry. On this account I would propose that this first discovered radicle composed of more than two elements should be named *proin* (from πρωΐ, the beginning of day) or *orthrin* (ὄρθρος, daybreak), terms from which names like *proic acid*, *orthric acid*, *proic chloride*, *orthric chloride*, etc., could be readily derived."

Wöhler remained in Cassel for nearly five years In the autumn of 1835 died Stromeyer, Professor of Chemistry in the University of Göttingen. Opinions were divided as to his successor: the choice lay between Liebig and Wöhler. Eventually Wöhler was selected, and entered on his work at Göttingen in the early part of 1836. He was succeeded at Cassel by Bunsen, who was at that time *privat docent* in Göttingen. In the October of that year Wöhler was again ready for fresh work. He writes to Liebig: "I am like a hen which has laid an egg and straightway sets up a great cackling. I have this morning found how oil of bitter almonds containing prussic acid may be obtained from amygdalin, and would propose that we jointly undertake the further investigation of the matter, as it is intimately related to the benzoyl research, and it would seem strange if either of us should work alone again in this field, denn es lässt sich gar nicht absehen wie weit es sich erstreckt, und ich glaube es ist gewiss fruchtbar, wenn es mit Deinem Mist gedüngt wird. . . ." In a couple of days afterwards Wöhler was ready with the fundamental facts which constituted the basis of the research, and had sketched out its plan. He writes:—

"I have just made a most remarkable discovery in relation to the amygdalin. Since it appeared that

bitter almond oil might be obtained from amygdalin, it occurred to me that the one might be converted into the other by simply distilling almonds with water by an action similar to that of a ferment upon sugar, the change in this case being due, in all probability, to the albumen in the almonds. And this idea seems to be completely established. The facts are as follows :—

"1. Amygdalin, dissolved in water and digested with a bruised sweet almond, begins almost immediately to smell of bitter almond oil, which after a time may be distilled off in such quantity that it would appear that the amygdalin was wholly transformed into it.

"2. A filtered emulsion of sweet almonds produces the same effect.

"3. A boiled emulsion of sweet almonds, in which, therefore, the albumen is coagulated, affords not the smallest trace of oil with amygdalin.

"4. Bruised sweet almonds, covered with alcohol, and freed therefrom by pressure, transform, as before, amygdalin into bitter almond oil.

"5. Bruised peas, or the albumen they contain, give no oil with amygdalin.

"There are three points, therefore, to be ascertained—

"*a*. What is the substance in bitter or sweet almonds which, in contact with amygdalin and water, forms bitter almond oil ?

"*b*. Is the action by double decomposition or catalytic, like that of a ferment ?

"*c*. What is the other product which, in all probability, is formed in addition to the oil and prussic acid ?"

The merest tyro in organic chemistry to-day is familiar with the broad features of this investigation, and knows the answers which Liebig was able to give

to his friend's interrogatories. The third substance Liebig discovered to be sugar. Under the influence of a nitrogenised ferment, termed by Liebig and Wöhler *emulsin*, amygdalin, in presence of water, is decomposed into benzaldehyde (bitter almond oil), prussic acid, and sugar (glucose), thus :—

$$\underset{\text{Amygdalin.}}{C_{20}H_{27}NO_{11}} + \underset{\text{Water.}}{2H_2O} = \underset{\text{Benzaldehyde.}}{C_7H_6O} + \underset{\text{Prussic acid.}}{CNH} + \underset{\text{Glucose.}}{2C_6H_{12}O_6}.$$

It simply remains to explain why this reaction only occurs when the almonds are bruised and digested with water. Both the emulsin and the amygdalin exist together in the almonds, but are contained in separate cells, and are only brought into contact by the rupture of the cell-walls and the solvent action of the water. Amygdalin was the prototype of a large and important group of substances now classed together as the glucosides.

At the instigation of Wöhler, the friends again returned to the question of the chemical nature of uric acid, and the memoir which they eventually published on the subject is of the profoundest interest, not only to the chemist, but also to the physiologist. Uric acid, originally discovered by Scheele, was shown, in 1815, by William Prout, then a boy of nineteen, to be the main constituent of the solid excreta of reptiles; other chemists had succeeded in obtaining various derivatives from it; indeed, Prout himself had prepared from it the so-called purpuric acid, a substance which years after, as murexide, obtained a transitory importance in the arts as a colouring matter. But nothing was known concerning the constitution of the body or of its relations to its derivatives until Wöhler and Liebig attacked the problem. The extraordinary mutability of uric

acid, which had baffled and deceived previous investigators, was to Wöhler and Liebig the clue to a labyrinth leading to a veritable treasure-house, and the wonderful insight and rare analytical skill of these two great men were never more clearly indicated than in the way in which they trod this intricate maze. No fewer than fifteen new bodies were added to the list of chemical compounds, and these were correlated with the same masterly lucidity that was so strikingly exhibited in the memoir on the radicle of benzoic acid. Some of the greatest triumphs of modern chemistry are seen in the synthesis of organic bodies. That organic chemistry was about to advance along this line was clearly foreseen by Wöhler and Liebig. In opening their account of this, the last great work they did in common, they say: "From this research the philosophy of chemistry will draw the conclusion that the ultimate synthetical formation in our laboratories of all organic bodies, in so far as they are not organised (in so weit sie nicht mehr dem Organismus angehören), may be regarded as not only probable but as certain. Sugar, salicin, morphin will be artificially obtained. As yet we know nothing of the way by which this result is to be attained, inasmuch as the proximate materials for forming these bodies are unknown; but we shall come to know them."

Henceforth the friends worked but little together. Liebig's energies were spent in other directions, and Wöhler turned his attention to inorganic chemistry. Time allows only the very briefest mention of his more important discoveries in this department of the science. We have first his isolation of crystalline boron, and the preparation of the compounds of boron with aluminium and nitrogen, work done in concert with Sainte-Claire

Deville. The readiness with which boron unites with nitrogen, and the mode in which the compound may be decomposed, led Wöhler to a conception of the origin of boric acid and borax in the volcanic waters in which they are frequently found. In collaboration with Buff he discovered the spontaneously inflammable hydride of silicon, the analogue of marsh gas, the simplest of the hydrides of carbon, and thereby laid the foundation-stone of a superstructure, which in time to come may be only less imposing than that built up of the compounds of carbon. Many years ago Wollaston noted the presence in the slags from the iron blast-furnaces of beautiful lustrous copper-coloured cubes, which he assumed to be metallic titanium; Wöhler proved this substance to be a compound of carbon, nitrogen, and titanium, and showed how it might be obtained. Of all the elements known to the chemist up to the period of Wöhler's cessation from work, it may be safely averred that there was not one but had passed through his hands in some form or other. Now he was busy with chromium, then with cerium, next with uranium and the platinum metals; titanium, tantalum, thorium, thallium, tungsten—all came in for some share of his attention. Of the minerals and meteorites he analysed the number is legion; indeed, as Professor Hofmann says, whoever sent him a piece of meteoric iron gained his heart. His untiring activity was a continual source of wonder to his friends. "How happy art thou in thy work!" wrote Liebig on one occasion; "thou art like the man in the Indian fable who, when he laughed, dropped roses from his mouth."

The names of Liebig and Wöhler are now so closely intertwined in the history of chemistry that it is hardly possible to avoid comparing the men. Such

a comparison has already been drawn by one who of all others is most fitted to draw it. "Liebig," says Dr. Hofmann, "fiery and impetuous, seizing a new thought with enthusiasm, and giving to it the reins of his fancy, tenacious of his convictions, but open to the recognition of error, sincerely grateful, indeed, when made conscious of it,—Wöhler, calm and deliberate, entering upon a fresh problem after full reflection, guarding himself against each rash conclusion, and only after the most rigorous testing, by which every chance of error seemed to be excluded, giving expression to his opinion,—but both following the path of inquiry in their several ways, and both animated by the same intense love of truth! Liebig, irritable and quick to take offence, hot-tempered, hardly master of his emotions, which not unfrequently found vent in bitter words, involving him in long and painful quarrels,—Wöhler, unimpassioned, meeting even the most malignant provocation with an immovable equanimity, disarming the bitterest opponent by the sobriety of his speech, a firm enemy to strife and contention,—and yet both men penetrated by the same unswerving sense of rectitude! Can we marvel that between two such natures, so differently ordered, and yet so complementary, there should ripen a friendship which both should reckon as the greatest gain of their lives?"

Who can fully gauge the influence of such a personality as Wöhler's? How it was exerted on Liebig is indicated in the following letter:—

FRIEDRICH WÖHLER TO JUSTUS LIEBIG.

GÖTTINGEN, *9th March* 1843.

To make war against Marchand, or, indeed, against anybody else, brings no contentment with it, and is of little use to science.

. . . Imagine that it is the year 1900, when we are both dissolved into carbonic acid, water, and ammonia, and our ashes, it may be, are part of the bones of some dog that has despoiled our graves—who cares then whether we have lived in peace or anger; who thinks then of thy polemics, of the sacrifice of thy health and peace of mind for science?—Nobody. But thy good ideas, the new facts which thou hast discovered, these, sifted from all that is immaterial, will be known and remembered to all time. But how comes it that I should advise the lion to eat sugar?

It was thus in philosophic contentment, happy in his work, in his home life, and in his friendships, that Wöhler lived out his fourscore years and two. He made Göttingen famous as a school of chemistry; at the time of the one-and-twentieth year of his connection with the university it was found that upwards of 8000 students had listened to his lectures or worked in his laboratory. There was hardly an academy of science or a learned society which did not in some way or other recognise his services to science. He was made a Foreign Member of the Royal Society in 1854, a Corresponding Member of the Berlin Academy in 1855, Foreign Associate of the Institute of France in 1864, and in 1872 he received the Copley Medal from the Royal Society. He died on 23rd September 1882.

XI

JEAN BAPTISTE ANDRÉ DUMAS

A Lecture delivered to the Royal Dublin Society, March 1885.

Jean Baptiste André Dumas was born on 14th July 1800 at Alais, in the department of Gard, where his father held the position of town-clerk. After having passed through the small school of his native town, it was intended that young Dumas should enter the navy, but the disasters of 1814-15 put an end to the project, and he was eventually apprenticed to an apothecary in Alais. There was much that was congenial to the boy's tastes in such a calling. The bent of his mind towards natural science had already declared itself, and the career of a pharmacist seemed to offer opportunities for its pursuit. Moreover, there were many things in and about Alais to stimulate his interest in matters relating to chemistry. In its glass works, and manufactories of earthenware, in its limekilns, and its smelting-houses of lead and antimony, the young apothecary had occasion to observe the connection of the science with technical processes; and in his subsequent writings we find frequent mention of his early impressions.

Alais, however, did not hold Dumas long. He was barely sixteen years of age when he determined to

leave his native town; and he set out on foot for Geneva, where he had relatives, and entered the pharmaceutical laboratory of Le Royer.

Geneva was then, as now, a centre of academic life, and the boy found everything there to stimulate his intellectual activity and to quicken his thirst for knowledge. Gaspard de la Rive at that time lectured on chemistry, Pictet on physics, and De Candolle on botany—all honoured names in the history of science.

It was said of Dumas in later years that he seemed predestined to presidency, and we find him even in his student days taking a leading place in the various scientific associations and social gatherings of his fellows. They suggested that he should give them a course of instruction in experimental chemistry; and it was with the aid of a few glass tubes, a syringe for an air-pump, lamp chimneys made into gas jars, and a balance constructed by a watchmaker, that he made his *début* as professor. He came under the notice of Theodore de Saussure and of De Candolle; and probably at their instigation, or possibly prompted by his latent naval predilections, he sought to qualify himself for service in an exploring expedition. One outcome of this work of preparation was a monograph on the *Gentianæ*, compiled with a view of familiarising himself with the conceptions and terms of botanical science.

But Dumas soon turned into other paths. As his knowledge increased the range of his horizon widened. The brilliant discoveries of Davy, of Berzelius, and of Gay Lussac and Thénard, had fired his enthusiasm, and to an active vigorous mind like his to peruse their memoirs was to conceive of new problems and fresh fields of work. The chemical student of that period was not overburdened with text-books. Dumas was

nurtured on such fare as the great Treatise of Lavoisier and the *Statique Chimique* of Berthollet, and in the *Annales de Chimie et de Physique* he had a series of magnificent models of the art of scientific investigation. He was soon led to try his 'prentice hand at this art. The story of his first attempts was told by him to Professor Hofmann, to whom I am indebted not only for this, but also for many other accounts of incidents in Dumas's personal history. "When analysing various sulphates, and other salts of commerce, Dumas had observed that the water they contained was present in definite equivalents. He had not found this recorded anywhere, and had therefore taken great pains to establish the accuracy of his observations. When the investigation was finished, he went one morning early to M. de la Rive, and timidly submitted to him the manuscript embodying the results of his inquiry. Whilst glancing over it, M. de la Rive could not conceal his surprise. When he had come to the end he said to the young student, 'Is it you, my boy, who have made these experiments?' 'Certainly.' 'And they have taken you a good deal of time to perform?' 'Of course they have.' 'Then I must tell you that you have had the good fortune to meet Berzelius on the same field of research. He has preceded you; but he is older than you, and so you ought not to bear him ill-will on this account. . . . Come along and breakfast with me.'" The kindly feeling thus shown to Dumas by his teacher never subsequently failed, and on more than one occasion De la Rive gave him substantial proof of his friendship.

Nor was his next excursion along the road of discovery attended with more success. "He thought that, knowing the atomic weight of a solid or liquid

body, and likewise its density, it might be possible to arrive at the volume of the solid or liquid atom. He was thus led to determine, with great accuracy, the density of a number of simple and compound substances, the purity of which could be depended upon. Having worked for some time, he drew up a paper upon the subject, which was presented to M. de la Rive. But his friend, though admitting the novelty of the point of view from which the question was treated, did not encourage him to pursue this line of research. Young Dumas was rather disheartened when he left his patron. 'The first time,' he said to himself, 'my experiments were good, but they were not new; this time they are new, but they do not appéar to be good. I shall have to try again.'" Dumas was thus the forerunner along a line of research which is inseparably associated with the name of Hermann Kopp, whose work on the specific volumes of solid and liquid substances constitutes one of the classics of chemical physics.

Dumas's name first appears in chemical literature when he was eighteen years of age, and in connection with Coindet's method of healing that extraordinary enlargement of the thyroid gland known as *goître*, which is so prevalent in certain parts of Switzerland. Prior to the year 1818, the most successful mode of treating this disease was by the employment of carbonised sponge, in which Coindet, from its *habitat*, was led to suspect the presence of the element iodine, which Courtois had discovered some six years before in the liquid obtained by the lixiviation of burnt sea-weed. Dumas, at Dr. Coindet's request, examined sponges for iodine; he detected the presence of the new element, and suggested its employment as a tincture, as potassium iodide, and as iodised potassium iodide, all of which

preparations are nowadays well established remedies in the treatment of *goître* and other glandular swellings.

Dumas's association with Coindet led to his being requested by the physiologist Prévost to undertake the isolation of the active principle of digitalis, the foxglove of our fields, which was introduced into therapeutics as far back as the sixteenth century. The methods of organic chemistry in vogue at the time were, however, utterly inadequate to effect the separation of a substance which is of so indefinite a character that even to-day, despite a dozen memoirs on the subject, we have still much to learn respecting its chemical nature. But the connection with Prévost led to labours in physiology and physiological chemistry which were productive of the most fruitful results.

Prévost and Dumas clearly recognised that it was on the lines indicated by Spallanzani, with whom the study may be said to have taken its rise, that comparative physiology would most rapidly progress. A host of questions, which the methods of the professed physiologists of the day, as represented by Magendie, left untouched, suggested themselves to the two naturalists. What, for example, is the proximate nature of blood, and what is the function of its various constituents? What is the structure of the red-blood cell, and how does it differ in various animals? The answers which Prévost and Dumas were able to give to these queries are to be found in a paper in the *Bibliothèque Universelle* for 1821. Their experiments on the transfusion of blood, made at the period when the death of the Princess Charlotte excited a feeling of profound sorrow throughout Europe, attracted considerable attention, and gave fresh interest to the question of the possibility of prolonging human life by the process.

The mode of secretion of the waste nitrogen of the tissue and the seat of the formation of urea were next attacked. The particular question which they set themselves to answer was, in fact, part of the general problem of the function of the organs of secretion—whether, in fact, these organs actually generate the products which they separate from blood or lymph, or whether they merely act in eliminating these products. By experiments made on nephrotomised animals, Dumas and Prévost were able to demonstrate that the urea continued to be produced within the system, and could be detected in the blood. Whether the conclusions of the Geneva naturalists will continue to be held by physiologists in the future is doubtful, and the question of the true function of the kidneys is still far from being definitely settled.

The chemistry of embryology next attracted the attention of Prévost and Dumas, who must, therefore, be regarded as the immediate precursors of Baer, whose great work on the genesis of the ovum in the mammalia appeared some three years after the publication of their papers. Their observations on the chemical changes accompanying the development of the chick in the egg deserve notice, as embodying the first exact statements on a subject which has still its attractions for modern physiologists. Another of their joint papers, on the phenomena accompanying the contraction of muscular fibre, which they suggested might be explained by the aid of Ampère's discovery of the mode of action between two parallel electric currents flowing in the same direction, may also be mentioned in connection with the hypothesis of the identity of the nervous principle with electricity.

The appearance of Biot's *Treatise on Physics* had a

considerable, even if an indirect, influence in determining the direction of much of Dumas's earlier work. He had been struck, as many subsequent chemists have been, with the somewhat arbitrary and even irrational mode in which physicists occasionally select substances to form the groundwork of an investigation intended to elucidate a general physical law. For example, Dumas found that Biot had attempted to develop the law of the thermal expansion of liquids by the aid of Deluc's observations on the change of volume of the fixed oils by heat—substances confessedly impure, and mixtures of very dissimilar bodies. With a view of obtaining more definite information, Dumas had proposed to himself to study the thermal expansion of some one group of correlated substances, each member of which should be a homogeneous substance—that is, a chemical individual—capable of being obtained in a state of purity. He selected as the basis of his work the class of bodies known to chemists as the *compound ethers*. The research, however, developed in a direction altogether different from that originally intended. Unexpected difficulties in the way of obtaining these compound ethers in a state of sufficient purity presented themselves; and no rigorous guarantee of their individuality was possible with the means of analysis at that time known. The analytical results obtained by Dumas were, however, sufficiently precise to make him assured of one fact—viz., that the mode in which these substances had hitherto been viewed by chemists was at variance with their true nature. At that period the ethers were assumed to be compounds of alcohol with the anhydrous acids. Dumas, however, pointed out that alcohol could not be regarded as a proximate principle of these bodies —a view which, as we shall see immediately, he after-

wards developed in a research which exercised a very powerful influence upon the progress of organic chemistry.

Dumas was now twenty-two years of age. He was apparently settled at Geneva, and at the pharmacy of Le Royer, and was rapidly securing for himself a recognised position in the intellectual society of the town, when a very little incident changed the whole direction of his career. There is, we know, a tide in the affairs of men which, taken at the flood, leads on to fortune. And the tide in Dumas's affairs bore him towards Paris. How this happened Dumas has himself related. "One day," he said, "when I was in my study completing some drawings at the microscope, and it must be added, rather negligently attired, in order to enable me to move more freely, some one mounted the stairs, stopped on my landing, and gently knocked at the door. 'Come in,' said I, without looking up from my work. On turning round I was surprised to find myself face to face with a gentleman in a bright blue coat with metal buttons, a white waistcoat, nankeen breeches, and top-boots. This costume, which might have been the fashion under the Directory, was then quite out of date. The wearer of it, his head somewhat bent, his eyes deep-set but keen, advanced with a pleasant smile, saying, 'Monsieur Dumas?' 'The same, sir; but excuse me.' 'Don't disturb yourself—I am M. de Humboldt, and did not wish to pass through Geneva without having had the pleasure of seeing you.' Throwing on my coat, I hastily reiterated my apologies. I had only one chair. My visitor was pleased to accept it, whilst I resumed my elevated perch on the drawing stool. Baron Humboldt had read the papers published by M. Prévost and myself on Blood, which had just appeared in the *Bibliothèque Universelle*, and was

anxious to see the preparations I had by me. His wish was soon gratified. 'I am going to the Congress at Verona,' said he, 'and I intend to spend some days at Geneva, to see old friends and to make new ones, and more especially to become acquainted with young people who are beginning their career. Will you act as my cicerone? I warn you, however, that my rambles begin early and end late. Now, could you be at my disposal, say, from six in the morning till midnight?' This proposal, which was of course accepted with alacrity, proved to me a source of unexpected pleasure. Baron Humboldt was fond of talking; he passed from one subject to another without stopping. He obviously liked being listened to, and there was no fear of his being interrupted by a young man who, for the first time, heard Laplace, Berthollet, Gay Lussac, Arago, Thénard, Cuvier, and many others of the Parisian celebrities spoken of with familiarity. I listened with a strange delight; a new horizon began to dawn upon me. . . . Sometimes he turned to science, and then astronomy and physics, chemistry, and the natural history branches would in rapid succession come in for their share in the dialogue, or rather monologue, which, spoken in a low, somewhat monotonous tone, would have scarcely appeared impressive, had it not been for some waggish pleasantry which now and then escaped, as it were, involuntarily. But, at any rate, if his voice failed to be effective, the glance of his eye was sufficient to rivet his hearer's attention.

"At the end of a few days Baron Humboldt left Geneva. After his departure the town seemed empty to me. I felt as if spell-bound. The memorable hours I had spent with that irresistible enchanter had opened a new world to my mind. I had been more especially

impressed with what he had told me of Parisian life, of the happy collaboration of men of science, and of the unlimited facilities which the French capital offered to young men wishing to devote themselves to scientific pursuits. I began to think that Paris was the only place where, under the auspices of the leaders of physical and chemical science, with whom, I had no doubt, I should soon become acquainted, I might hope to find the advice and assistance which would enable me to carry out the labours over which I had been pondering for some time. My mind was soon made up; I must go to Paris."

Before a twelvemonth had elapsed Dumas found himself in Paris, and in a very short time he was an active participator in the intellectual life of the capital. Almost at the very outset of his career in Paris he had the rare fortune to become acquainted with two men of about his own age, with whom he was subsequently on terms of the closest intimacy, and who exercised a very great influence on the development of his scientific activity. The one was Adolphe Brongniart, the botanist —the other was Milne Edwards, the physiologist.

Of the manner in which he was received by the leaders of science in the French capital, we may judge from the following little story:—it was on the occasion of his *debut* in the Academy of Sciences. He had finished the reading of the paper by Prévost and himself on Muscular Contraction, of which mention has been already made, and was about to retire, when a white-haired man of a dignified countenance rose on the other side of the table and approached him. "Monsieur Dumas, will you do me the honour of dining with me on Wednesday next?" The invitation was accepted, and after the exchange of a few civilities the veteran

savant returned to his place. "With whom am I to dine?" asked Dumas of a bystander. "Did you not know—it is M. de Laplace?" This was the beginning of a friendship which ceased only with Laplace's death. Madame la Marquise de Laplace survived her husband many years; it is an indication of the warmth and ordiality of Dumas's relations with the household, that he should have been requested, as the friend of the family, to supervise the publication of the magnificent edition of the works of the author of the *Mécanique Céleste* which his son and grand-daughter gave to the world as the most enduring monument to the genius of their illustrious progenitor.

Dumas was now admitted to that brilliant galaxy of men of science—the most brilliant of any age or country, which at that period was the glory of France —Laplace, Berthollet, Vauquelin, Gay Lussac, Thénard, Alex. Brongniart, Cuvier, Geoffroy St. Hilaire, Arago, Ampère, Poisson. That constellation has set—

> the world in vain
> Must hope to look upon their like again.

But how did our hero live? With what did he occupy himself? Thanks to his friends and patrons, he had not long to wait for a congenial occupation. The place of *Répétiteur de Chimie* at the École Polytechnique being vacant, Dumas was appointed at Arago's suggestion. Almost immediately afterwards the professorship of chemistry at the Athenæum, an association somewhat similar in design and character to that which I have the honour of addressing [the Royal Irish Academy], fell vacant by the resignation of Robiquet—a name known in the history of chemical science as the discoverer of the dye *alizarin*,—and

Dumas, at Ampère's instigation, was elected to the chair. He was now installed in a laboratory whose traditions he had to maintain. As Thénard's assistant he might be supposed to be in the enjoyment of unlimited facilities for research, but great was his disappointment to find that all that remained of the magnificent equipment of which Gay Lussac and Thénard had made such signal use was so much of the apparatus and preparations as could be employed in the demonstrations in a course of lectures on general chemistry. But even if the instruments of precision had been there, the time in which to use them was wanting. Dumas found that his offices were no sinecures. The work involved in the preparation of the lectures and in their experimental illustration was considerable, and the audiences of the Athenæum were exacting. Moreover, he had embarked in a literary enterprise with Audouin and Brongniart: 1824 saw the foundation of the *Annales des Sciences Naturelles.* At the same time, too, he had projected his great work, *On Chemistry Applied to the Arts,* of which the first volume appeared in 1828. And there was still a third and perhaps the most cogent reason—he was engaged to be married. In 1826 he was united to Mdlle. Herminie Brongniart, the sister of his coadjutor Adolphe, and the daughter of Alexandre Brongniart, the geologist.

Dumas's first contribution to pure chemistry appeared in the *Annales de Chimie et de Physique* in 1826. It had reference to the nature of the spontaneously inflammable gas which is evolved by the action of water and of hydrochloric acid upon calcium phosphide, a substance which to-day finds application as a signal fire. His next contribution was a note on the remark-

able flash of light which attends the solidification of fused boric acid.

These matters have their interest from the fact that they are among Dumas's earliest papers. Far more important, however, was his classical memoir on "Some Points in the Atomic Theory"—classical by reason of its influence on the development of chemical philosophy. The inquiry opens with the precise conceptions which form the groundwork of modern chemical theory. Dumas for the first time recognises the relations of the doctrine of Avogadro to the atomic theory of Dalton. "I am engaged," he says, "in a series of experiments intended to fix the atomic weights of a considerable number of bodies by determining their density in the state of gas or vapour. There remains in this case but one hypothesis to be made, which is accepted by all physicists. It consists in supposing that in all elastic fluids observed under the same conditions, the molecules are placed at equal distances, *i.e.* that they are present in them in equal numbers. An immediate consequence," he goes on to say, "of this mode of looking at the question has already been the subject of a learned discussion on the part of Ampère, to which, however, chemists, with the exception, perhaps, of M. Gay Lussac, appear to have given as yet but little attention. It consists in the necessity of considering the molecules of the simple gases as capable of a further division—a division occurring at the moment of combination and varying with the nature of the compound." Dumas, in a word, realises the conception of the difference between the chemical atom and the molecule which we all accept to-day. The good seed, however, fell on unprepared ground, and more than a quarter of a century was

needed for it to germinate. The immediate value of Dumas's memoir lay in its description of the particular method of determining vapour-density with which his name is associated. The simplicity of this method is one of its great merits ; it instantly found its way into chemical laboratories, and has proved of incalculable service to science. A number of determinations of the vapour-densities of various substances were made by Dumas, who thereby established the relative weights of the molecules of phosphorus, arsenic, and boron. Another result of Dumas's work, as given in this memoir, was the discovery of the real nature of silica, and therefore, indirectly, of vast numbers of substances occurring in the mineral kingdom. His enunciation of the constitution of the silicic acid brought him, however, into collision with Berzelius. At that period the influence of the great Swedish chemist was supreme ; his opinions had all the force of legislative enactments. Immediately before the publication of Dumas's paper Berzelius had given to the world his views on the constitution of the natural silicates, and his conception of the chemical nature of silicic acid was very different from that of the young French chemist. Berzelius defended his formula for silicic acid with considerable warmth ; he advised Dumas to be more careful in interpreting his experiments, and warned him not to allow himself to be carried away by the evidence of a single experiment. The view which Dumas promulgated has, however, prevailed, and the opinion that the molecule of silica contains two atoms of oxygen is the settled conviction of modern chemistry.

We have seen that whilst in Geneva Dumas had attempted to prepare the compound ethers with a view of studying some of their physical properties, but that

he had been deterred from the prosecution of his work by the difficulty of obtaining these substances in a state of purity. Moreover, the analyses of the compounds which he had been able to prepare did not appear to be in conformity with the current opinions as to the nature of these substances. In conjunction with his assistant Boullay he now resumed their study. At the period of these researches common alcohol was regarded as a combination of one volume of olefiant gas with one volume of water; common ether as a combination of two volumes of olefiant gas with one volume of water. Hence ether might be regarded as derived from alcohol by the abstraction of water.

Dumas and Boullay began their investigation by supplying new experimental data in confirmation of this view of representing the composition of these substances. In the notation of to-day their formulæ were—

Alcohol . . . . $C_2H_4 . H_2O$.
Ether . . . . $2C_2H_4 . H_2O$

In the preparation of ether from alcohol there is a product formed, known even before the time of Dumas, and called sulphovinic acid; the mode of origin of this compound is explained by Dumas and Boullay, and represented by an equation identical with that which we now employ.

These researches supplied us for the first time with a number of well-ascertained facts respecting one of the most important groups of organic compounds. But this was not by any means their chief merit. Dumas and Boullay conclude their memoir with a suggestive synoptical review of the relation of these compounds to the salts of ammonia. The nature of this relationship may be seen from the following table :—

Hydrochlorate of bicarburetted hydrogen	$C_2H_4 . HCl$.
Nitrate of bicarburetted hydrogen	$C_2H_4 . HNO_2$.
Sulphate of bicarburetted hydrogen	$C_2H_4 . H_2SO_4$.
Alcohol	$C_2H_4 . H_2O$.
Ether	$(C_2H_4)_2 . H_2O$.
Ammonia hydrochlorate	$NH_3 . HCl$.
Ammonia nitrite	$NH_3 . HNO_2$.
Acid ammonia sulphate	$NH_3 . H_2SO_4$.
Aqueous ammonia	$NH_3 . H_2O$.

In a word, these derivatives of alcohol may be considered to be compounds containing olefiant gas, in the same way that ammonia is held to be contained in the ammoniacal salts. This view of the constitution of the ethers was at first opposed by Berzelius, but was subsequently adopted by him, and became known as the *etherin* theory, from the name which he gave to the radicle C_2H_4. The etherin theory, however, never gained general acceptance. Dumas and Boullay, in fact, sought to make it too comprehensive. Moreover, the analogy with the ammoniacal salts broke down from the circumstance that it was not possible to build up the compound ethers by bringing together their proximate constituents, as in the case of the ammoniacal salts. As regards the latter point modern chemistry has shown that such synthetical formations are actually possible; and had the facts of to-day been known to the contemporaries of Dumas the etherin theory might have had a longer lease of existence than it actually enjoyed. The etherin theory had, however, the great merit of familiarising chemists with the conception of organic radicles. It further indicated that the reactions of organic chemistry are truly comparable with those of inorganic chemistry, and can be represented by equations as precise and as simple in character as those which express the chemical transformations of the mineral kingdom. Berzelius, by his

adoption of the etherin theory, removed the barrier which he himself had set up between the two main divisions of the science. In a living structure, said Berzelius, the elements obey laws totally different from those which regulate the formation of compounds in the inanimate world, and the products which result from the mutual action of these elements differ from those which are presented to us in inorganic nature. To-day we recognise no such distinction; the doctrine of the specific action of a so-called vital force derives no support from chemistry.

But the analogy between the compounds of etherin and ammonia was fruitful in another direction. Some years after the publication of Dumas and Boullay's memoir, the late Sir Robert Kane drew attention to the fact that the constitution of the alcoholic derivatives could be better explained on the assumption that they contained a radicle which differed from etherin, in the same manner that the ammonium of Ampère and Berzelius differed from ammonia. In the Dublin *Journal of Medical and Chemical Science* for 1833 there is a paper by Sir Robert Kane on the Theory of the Ethers, in which this view is developed, and in which he denotes by the name of *ethereum* the hypothetical body formed by the union of etherin with hydrogen (following Berzelius, who termed the combination of ammonia with an atom of hydrogen, *ammonium*), and in the paper the constitution of some of the more important of the alcoholic derivatives is expressed by its aid. Fifty years ago it would seem that Dublin had such little interest in questions relating to chemical theory that, in a subsequent paper, Sir Robert Kane was fain to confess that his speculations were regularly a subject of amusement and ridicule in the chemical

circles of this city. But the whirligig of time brings about its revenges; and to-day my friend the Professor of Chemistry in Trinity College, Dublin, in common with his fellow-chemists, explains the relations of alcohol to its many derivatives by the aid of the conception of Kane.

An infallible test of the value of a scientific paper is afforded by the number and variety of the issues it suggests; and measured by this criterion, the memoir of Dumas and Boullay must rank among the classics of organic chemistry. In the attempt to isolate ether from the ethereal salts Dumas discovered the nature of oxamide and of ethyl oxamate, prototypes of compounds of great theoretical interest to the chemist.

The paper on Wood-Spirit, which Dumas published in conjunction with Péligot in 1832, may be here referred to. It was known from the researches of Taylor, made in 1812, that when wood is heated in closed vessels it yields, in addition to acetic acid, a very volatile and inflammable fluid, termed by Taylor *pyroligneous ether*, a substance which, in many of its characters, resembles ordinary alcohol, but which differs from it by not yielding ether on treatment with sulphuric acid. The nature of wood-spirit had been the subject of investigation by many chemists since the time of Boyle, but without any definite result until Dumas and Péligot succeeded in isolating its main constituent. By determinations of its vapour-density, and by the study of its behaviour towards various reagents, they established its composition and chemical nature. They showed that the body was in fact analogous to common alcohol; that it gave rise to compound ethers, and to an acid corresponding with acetic acid. These views of its relations they embodied

in the name which they gave to it of *methyl alcohol*—the word *methyl* being derived from the Greek, and signifying the *wine of wood.*

Closely following on the publication of this memoir was another on the nature of Spermaceti, which had already been made the subject of inquiry by the veteran Chevreul. Dumas and Péligot showed that spermaceti —the fatty substance found in cavities in the head of the sperm-whale—yields on saponification a substance which, from its chemical transformations, they regarded as akin to common alcohol, but which differs from that body and from methyl alcohol by multiples of the number of carbon and hydrogen atoms they contain. The full value of these discoveries was first clearly indicated some years later by Gerhardt. Methyl alcohol is, in fact, the first term of a series of homologous substances, of which ordinary alcohol and cetyl alcohol, —the alcoholic substance contained in spermaceti,—are widely separated members. The discovery of these alcohols led directly to the classification of organic compounds in homologous series—a mode of classification which has been singularly valuable in enriching organic chemistry with substances whose existence would otherwise have been unsuspected.

Dumas was now in the full tide of his chemical activity, and memoir followed memoir in quick succession. His work was distinguished by its universality; it ranged literally over every department of chemistry, whether pure or applied, organic or inorganic. Now it was on the nature of solution, then on the determination of specific heat; next on the composition of the more important varieties of glass known to commerce, the nature of minium, the preparation of the metal calcium, the compounds of phosphorus, the treatment of iron

ores, the composition of the materials used in the thirteenth-century frescoes—these, and a host of other problems, attracted his attention. During a tour in Switzerland he was shown, by the apothecary Pagenstecher, a sample of essential oil obtained by distilling the flowers of meadow sweet, *Spiræa ulmaria*; this he promptly recognised as salicyl aldehyde. A chance observation of an efflorescence on the walls of a bath-house at Aix-les-Bains led to the discovery of the mode of oxidation of sulphuretted hydrogen to sulphuric acid by the action of porous materials containing air.

We must not, however, so summarily dismiss certain of the memoirs, for it is no figure of speech to say of some of them that they were momentous in their consequences. The origin of one of these researches—perhaps the most fruitful in its results—was somewhat singular.

One evening, at a *soirée* at the Tuileries, given during the ill-starred reign of Charles X., the guests were greatly annoyed by the presence of acid vapours diffused through the air of the apartments; these were observed to be due to the burning of the wax candles. Dumas's father-in-law, Alexandre Brongniart, was at the time director of the procelain manufactory at Sèvres, and hence, by tradition, was regarded as the chemical adviser of the royal household; he was commissioned, therefore, to inquire into the cause of the peculiar behaviour of the candles. The actual investigation was intrusted to Dumas, who had no difficulty in determining that the acid vapours were caused by hydrochloric acid, produced by the combustion of wax which had been bleached by chlorine. The amount of chlorine contained in the wax was too considerable for it to be regarded as an accidental contamination;

it was evident that it had entered into combination with the organic matter. This circumstance led Dumas to study the action of chlorine upon organic bodies in greater detail than had hitherto been done, with results which have exerted a profound influence upon the development of organic chemistry.

At the time that Dumas began these researches, the electro-chemical theory of Berzelius reigned supreme. When water is subjected to electrolysis, the hydrogen makes its appearance at the negative electrode; whilst the oxygen is evolved at the positive pole. Hydrochloric acid, when electrolysed, is likewise decomposed, the hydrogen being liberated at the negative pole, and the chlorine at the positive pole. Moreover, when a metallic chloride is decomposed by electrolysis, the metal makes its appearance at the negative pole, whilst the chlorine is evolved at the positive pole. Hence hydrogen and the metals were termed electro-positive elements; chlorine and oxygen electro-negative elements. Facts of this order were generalised by Berzelius into a theory of chemical combination. According to this theory chemical combination is the result of the union of substances of different electric polarities. These substances might be elementary atoms, or they might be groups of atoms going in and out of combination like simple bodies. All compound substances were regarded as formed by the juxtaposition of two proximate constituents which might be simple or compound; if compound, they were also formed by the union of two components, and so on. This dualistic theory explained a sufficient number of facts to cause its acceptance by the entire chemical world prior to 1832. It was held that the electro-chemical character of an element or compound radicle determined its behaviour and influenced

the nature of the compounds into which it entered. Potassium oxide, K_2O, was a basic substance formed by the union of electro-positive potassium with electro-negative oxygen; this binary compound united with SO_3 to form a binary compound of the second order, $K_2O + SO_3$. Water, H_2O, was also formed by the union of the electro-positive hydrogen with electro-negative oxygen, and it too was regarded as a basic substance, inasmuch as it could combine, say, with SO_3 to form a body $H_2O + SO_3$, analogous in constitution to potassium sulphate. At that time the formulæ of these substances were almost universally written in this manner, so as to imply their binary constitution.

Dumas observed that when chlorine acts upon a number of organic bodies, it turns out the hydrogen, atom after atom, and takes the place of each hydrogen atom so expelled—that is, the atom of electro-negative chlorine occupies the same position as the atom of electro-positive hydrogen. As an historical fact, it should be noted, this circumstance had been observed by others. Gay Lussac had noticed that prussic acid when treated with chlorine loses hydrogen, and for each atom of hydrogen lost, an atom of chlorine was gained. Faraday, too, had observed that Dutch liquid, $C_2H_4Cl_2$, is by the continued action of chlorine converted into C_2Cl_6, or $C_2Cl_4Cl_2$. But these facts were too few and isolated for their bearing on the dualistic theory of chemical combination to be perceived. Their significance, however, was quickly appreciated by Dumas; he greatly extended them, and throwing down the gauntlet he boldly attacked the generalisation of Berzelius. "These electro-chemical conceptions, this special polarity which has been assigned to the elementary atoms,—do they really rest on such evident

facts that they are to be accepted as articles of faith? Or if we regard them only as hypotheses, do they possess the property of adapting themselves to facts; are they capable of explaining them; can we assume them with such a complete certainty that in chemical investigations they appear as useful guides? We must admit," replies Dumas, "that such is not the case."

It is impossible to set forth here the array of facts with which Dumas and the French school of which he was the leader confronted the arguments, and even the scorn and ridicule, of Berzelius and his adherents. Every hypothesis advanced by the Swedish chemist required to be sustained by fresh assumptions, until the binary theory absolutely sank under the load of contradictions and anomalies it had to carry. The followers of Berzelius gradually deserted him, and the electrochemical theory, which he had elaborated with such zeal and argumentative power, fell to pieces.

But the conception of *metalepsis*, as the doctrine of substitution was styled, was even more constructive than destructive in its results. A revolution which brings nothing but disorder is baneful. It is the chief glory of the doctrine of substitution that it has been the fruitful mother of speculations by which the superstructure of modern chemical philosophy has been raised.

By a natural and logical process of transition, the doctrine of substitution became gradually merged into the doctrine of types. The ideas of Dumas were amplified by Laurent and Gerhardt; Laurent, indeed, pushed the theory of substitution in a direction which Dumas was at first disinclined to follow, by boldly stating that chemical substances owed their properties much less to the nature of their elementary atoms than to the mode of arrangement of these atoms within the

compound. The creation of types by Gerhardt was the necessary outcome of substitutional conceptions, and out of the types, which in the hands of Williamson and Odling have rendered such signal service to chemistry, have sprung our present notions of the intrinsic difference in the atom-fixing power of the elementary and compound radicles—that is, the doctrine of valency—which we owe to Frankland. If we compare the chemistry of to-day with that of the stirring times when Dumas, half a century ago, was matched almost single-handed against the German school—against such Titans as Berzelius, Liebig, Wöhler—we are amazed at the wealth of material which has been opened out. The change, thus directly or indirectly traceable to the labours and conceptions of Dumas, is as great or even greater than that achieved by the overthrow of the Phlogistians. If Lavoisier was the author of the first French Revolution in Chemistry, Dumas was the creator of the second. Liebig, fiery and impulsive as he was, was a generous-hearted opponent, and it was a significant compliment to his quondam antagonist when, on being taxed with his desertion of organic chemistry, he replied: "I have withdrawn from organic chemistry, for with the theory of substitution as a foundation, the edifice of chemical science may be built up by workmen." This remark was uttered post-prandially, it is true, yet it had a meaning which was fully realised and appreciated by the company to which it was addressed.

But victory to Dumas meant a fresh campaign. We have seen how he had been led to the conception of the homologous series of the alcohols by the discovery of methyl alcohol, and the alcohol which functions in spermaceti. "The recognition of an alcohol," he said, "enriches organic chemistry with a series of compounds

comparable with those with which mineral chemistry is endowed by the discovery of a new metal. As yet we know only how to transform an alcohol into the corresponding acid; of equal if not greater importance would be the inverse process, the conversion of acids into alcohols. There can be no doubt that before long this problem will also be solved."

Dumas then pointed out the existence of homology in the fatty acids—in the series of acids of which palmitic and margaric acids, no less than acetic acid, are members. In 1843 he showed that as many as fifteen acids might be assumed to exist between margaric acid and formic acid,—the acid obtained by the oxidation of methyl alcohol—of which nine were already known. When the missing links in the chain were indicated, their discovery followed almost as a matter of course.

There is no branch of technical chemistry which has made more rapid progress or which exhibits more striking triumphs than that concerned with the tinctorial arts. In the minds of many people nowadays, the operations of modern chemistry are confined to the discovery of coal-tar colours. Dumas, who worked in every field of chemistry, has left his mark on this department of the science. The nature of indigo, and of the relation of white indigo to blue indigo, and of the chemical changes occurring in the indigo vat, were first correctly traced by him. One of the important dyeing materials of these later days is picric acid, a body of a beautiful yellow colour and of great dyeing power. It is obtained by the action of nitric acid upon phenol, better known perhaps as carbolic acid. The true nature of picric acid was first interpreted by Dumas; it is phenol, in which three atoms of hydrogen are replaced by an equivalent amount of the compound radicle NO_2.

Phenol . . . C_6H_6O.
Picric acid . . $C_6H_3(NO_2)_3O$.

Hence the body is now known by the systematic name of *trinitrophenol.*

The vegetable dyes contained in the lichens and in archil were also the subject of investigation by him. He established the formula of orcinol and of the beautiful red compound which is obtained from it by the action of ammonia and oxygen, and which is known as orceïn.

Orcinol . . . $C_7H_8O_2$.
Orceïn . . . $C_7H_7(NO_2)O_2$.
$C_7H_8O_2 + NH_3 + O_3 = C_7H_7NO_3 + 2H_2O$.

He investigated also the essential oil of mustard; the products obtained by the distillation of resin; the constitution of the more important vegetable acids, such as citric and tartaric acid; and, in conjunction with Pelletier, the composition of such of the vegeto-alkaloids as were at that time known.

We may here allude to the service rendered to analytical chemistry by the process which Dumas devised for the determination of the amount of nitrogen in organic substances. This method is in frequent use to-day, and is characterised by accuracy and simplicity, and by the fact that it is of general application. It consists in heating the organic substance, mixed with oxide of copper, in a glass tube, passing the evolved gases over metallic copper, and collecting them in a measuring vessel filled with caustic potash. The carbonic acid gas is absorbed by the potash, and from the volume of the residual nitrogen the weight of that element in the organic compound is readily calculated.

The quantity of carbon contained in an organic body is almost invariably ascertained by heating the substance

with an appropriate oxidising agent, collecting and weighing the carbonic acid formed, and calculating the amount of carbon from the ratio of the oxygen to carbon. Everything depends upon the accuracy with which this ratio is established, and accordingly some of the greatest masters of analytical chemistry, beginning with Lavoisier, have made it the subject of the most rigorous experiments. In conjunction with his pupil Stas, Dumas carried out a series of very accurate determinations of the amount of carbon which unites with oxygen, with the result of showing that the numbers in vogue at the time were inaccurate. By burning diamonds, which constitute the purest form of carbon, in a stream of oxygen, they found that 12 parts of carbon united with 32 parts of oxygen to form 44 parts of carbon dioxide—a result confirmed by similar experiments made with graphite. This ratio is universally accepted by chemists to-day.

The amount of hydrogen in an organic body is also invariably ascertained by burning the hydrogen to water, and from the ratio of the weight of hydrogen to oxygen in the water calculating the quantity of hydrogen in the organic substance. The exactitude of the ratio is obviously all important. The volumetric composition of water had been definitely established by the experiments of Cavendish and of Gay Lussac and Humboldt, and the gravimetric composition could be calculated, provided that the relative weights of equal volumes of hydrogen and oxygen were accurately known. Dumas sought to solve the problem by directly combining oxygen and hydrogen—that is, by determining the weight of the water formed by the union of a known weight of oxygen. The weight of the water being known, together with that of the oxygen which was

contained in it, the difference is of course due to the hydrogen; and hence the ratio of weight in which the oxygen and hydrogen have combined together to form water could be ascertained.

The relative atomic weights of hydrogen, oxygen, and carbon are perhaps the most important of the fundamental factors of analytical chemistry. They are the constants most frequently needed in chemical calculations; they are indeed as necessary to the chemist in the identification of a vast number of chemical substances as are certain fundamental astronomical data to the mariner who seeks to ascertain his position at sea; and for these constants, determined with a precision and scrupulous regard to manipulative detail which have never been surpassed, chemistry will for ever remain indebted to Dumas.

It was characteristic of Dumas that, having thus established the composition of water, he should next turn his attention to that of air. Indeed the question of the gravimetric composition of atmospheric air followed as the necessary consequence of his experiments upon water. The volumetric composition of air was known with approximate accuracy, but the proportion by weight of its components could not be accurately determined by calculation so long as the relative weights of oxygen and nitrogen remained imperfectly ascertained. In conjunction with his friend Boussingault, Dumas now attacked the question of the ponderal composition of atmospheric air.

As the result of their experiments, it was ascertained that 100 parts by weight of atmospheric air, free from carbonic acid and aqueous vapour, contained 23 of oxygen and 77 of nitrogen. Now the volumetric ratio of the two gases might be determined, provided the

volume-weights of the nitrogen and oxygen were accurately known. On dividing each number by the specific gravity of the gas, as at that time determined, it was found that a notable deficiency occurred—far greater than was warranted by the errors incidental to the methods of experiment. Hence Dumas and Boussingault were led to doubt the correctness of the generally received values for the volume-weight of oxygen and nitrogen, and on a careful repetition of the experiments required to ascertain the specific gravities of these gases, they found numbers differing from those at that time accepted as accurate. This research, therefore, not only served to give precision to our knowledge respecting the gravimetric composition of air, but also led to the correct determination of the specific gravities of its components.

At the close of their memoir, Dumas and Boussingault entered into some interesting calculations respecting the influence of the numerous agencies which serve to affect the composition of the air. Among the causes which tend to abstract the oxygen from the air may be enumerated—(1) The respiration of animals; (2) The combustion of organic matter; (3) The decay and putrefaction of organic substances; (4) The disintegration of rocks; and (5) The ultimate oxidation of inorganic matter, as, for example, the conversion of the lower oxides of iron to the state of peroxides. In this last case oxygen would appear to be permanently lost to the atmosphere, since it is only in rare instances, as in the smelting of iron ore, that it can be again restored by processes of reduction. A very little calculation will serve to show that the reservoir of oxygen contained in the atmosphere is amply sufficient to maintain its numerous functions. If we suppose the atmosphere to

be put into a huge vessel, and suspended from the arm of a gigantic balance, it would require 581,000 cubes of copper, each having a side of 1 kilometre in length, to equipoise it. If we also assume that each individual consumes 1 kilogram of oxygen per day, and that the population of the earth is one thousand millions, and further, that the oxygen consumed in the respiration of other animals, and in the oxidation of organic matter, amounts to four times that required by man, and also that the oxygen disengaged by plants compensates only for the causes of diminution of oxygen not specified—then, even in this exaggerated case, the amount of oxygen abstracted from the air in a century would only amount to 15 or 16 of the copper cubes; or, in other words, the abstraction in a century would only be $\frac{1}{8000}$th of the whole quantity of oxygen contained in the air—an amount barely appreciable by the most exact eudiometric methods known to us.

These calculations have been revised, and their substantial accuracy confirmed, by Professor Le Conte. Certain of the data employed by Dumas and Boussingault were of necessity only imperfectly known at the time, but the main conclusion has not been disturbed by the more accurate knowledge which we possess to-day.

Probably the most important contribution made by Dumas to determinative chemistry,—important as regards its influence upon the development of chemical philosophy,—was his revision of the relative atomic masses of the elementary bodies. These values are not only the fundamental constants of chemistry; their characters and relations as mere numbers are of the highest significance in relation to the theory of the essential nature of matter. It was this philosophic

aspect of the question which mainly attracted Dumas, and which supported him through perhaps the most tedious and most exacting of all his researches. The determination of the atomic mass, even of a single element, if made in such a manner as to satisfy the requirements of modern science, necessitates a combination of analytical skill and manipulative dexterity with unlimited patience and an unbending resolution not to swerve from the highest ideals of quantitative accuracy. What, then, must be the strain both on the mental and moral energy of him who sets himself the gigantic task of redetermining the atomic weights of all the elements, with the precision required by contemporary science? It is not surprising that Dumas's determinations should require many years for their accomplishment. Even when they were finished, nearly two years were occupied in arranging the results for publication.

The immediate effect of their appearance was twofold. In the first place, they secured to Dumas a position on a par with that of Berzelius as a master of determinative chemistry; the Swedish chemist had devoted many years of his life to the same work, and all Europe regarded him as *facile princeps* in this particular domain. In the next place, it again brought the French school into direct antagonism with the followers of Berzelius. To Berzelius the different elements were so many different forms of matter; their molecules had nothing in common beyond their unalterability and their eternal existence. It had been pointed out that the numerical relations of the values for the atomic masses indicated the existence of some intimate connection among them, and Prout had timidly revived the doctrine of a *materia prima* to account for this connection; the elements, he said, are but modifications

of one primordial substance, and he sought for the experimental confirmation of this hypothesis in the circumstance that all the values might be regarded as multiples by integral numbers of that of hydrogen. There was nothing unphilosophic in such a notion; the physicist had established the idea of the correlation of the forces—electricity, magnetism, heat, chemical action, are but modifications of the same agent; and just as to-day we find that the conceptions of evolution have permeated every department of intellectual activity, it was but natural that the doctrine of the unity of force should be enlarged so as to include the idea of the unity of matter.

Dumas seems to have been attracted by this idea with peculiar force. The very nature of his previous work had in fact prepared his mind for its reception and fertilisation. To the man who had been one of the pioneers of organic chemistry, who had led the way in the inception of the notion of compound radicles,—bodies often absolutely dissimilar in their physical and chemical attributes, although composed of the same elements,—and who had familiarised himself and the world with the ideas of substitution and homology—to him there was nothing repugnant in the hypothesis of a *materia prima*; it indeed was the natural outcome of the whole of his philosophy. Dumas's re-examination of the experimental evidence on which Prout's hypothesis was based led him to modify that hypothesis in certain essential particulars. As the result of his determinations of the atomic weights of thirty elementary bodies, Dumas found that in twenty-two cases the atomic weights are multiples by whole numbers of that of hydrogen, in seven they are multiples by half, and in three of one-fourth of that value.

A number of questions suggested themselves to Dumas at the outset of these inquiries, to which his experiments seemed to afford definite answers. It had been already noticed that in a so-called family of elements, — *i.e.* in a group of bodies like chlorine, bromine, and iodine, or of metals like lithium, sodium, and potassium,—the members of which are possessed of many properties in common,—the individuals in the family show a remarkable gradational order in their atomic weights ; the atomic weight of sodium is almost exactly the arithmetic mean of the atomic weights of lithium and potassium, and the atomic weight of bromine is the mean of that of chlorine and iodine. Is this a natural law ? Are the chemical attributes of an elementary body a function of its atomic weight ? Affirmative answers to questions of this kind would obviously tend to strengthen the validity of Prout's hypothesis. A certain amount of positive evidence has been accumulated. Thus :—

$$\frac{\text{Li}+\text{K}}{2} = \text{Na, or } \frac{7+39}{2} = 23.$$

$$\frac{\text{K}+\text{Cs}}{2} = \text{Rb, or } \frac{39+133}{2} = 86.$$

$$\frac{\text{Ca}+\text{Ba}}{2} = \text{Sr, or } \frac{40+136{\cdot}4}{2} = 88{\cdot}2$$

$$\frac{\text{Be}+\text{Ca}}{2} = \text{Mg, or } \frac{9+40}{2} = 24{\cdot}5.$$

$$\frac{\text{Mg}+\text{In}}{2} = \text{Ga, or } \frac{24+113}{2} = 68{\cdot}5.$$

$$\frac{\text{P}+\text{Sb}}{2} = \text{As, or } \frac{31+120}{2} = 75{\cdot}5.$$

$$\frac{\text{P}+\text{Bi}}{2} = \text{Sb, or } \frac{31+209}{2} = 120.$$

Other examples of relationships of this order might be adduced, but these quoted are sufficiently numerous to indicate that some intimate connection would seem

to exist between the individual members of well-defined groups of elementary bodies; and that the absolute individuality postulated by Berzelius is, in all probability, not the order of nature.

Dumas also pointed out the existence of differences in the atomic weights of the elements in a family, similar to those exhibited by the molecular weights of homologous organic compounds. Thus :—

Li	7.
Na	$7+[1\times16]=23$.
K	$7+[2\times16]=39$.
O	16.
S	$16+[1\times16]=32$.
Se	$16+[4\times16]=80$ (79).
Te	$16+[7\times16]=128$ (126·3).
Mg	24.
Ca	$24+[1\times16]=40$.
Sr	$24+[4\times16]=88$ (87·2).
Ba	$24+[7\times16]=136$ (136·4).

The "numerics" of the chemical elements have received much attention since the date of Dumas's memoir, and there is a conviction in the minds of many chemists that their relationships are determined by some natural law concerning which we have as yet but the merest inkling. Prout's hypothesis—or rather the fundamental conception at the base of it—is still an open question. Since the time of the publication of Dumas's papers—that is, during the last quarter of a century—a number of atomic weights have been redetermined with the very highest degree of precision attainable by modern chemical science, with the result of showing that the hypothesis is in a stronger position than ever.

The question has been examined from many different

sides; the spectroscope, for example, has shown the possibility of progressive dissociation at very high temperatures among the forms of matter which we call elements, and the discovery by Victor Meyer of the breaking up of the molecule of iodine and of bromine has demonstrated the existence of atomic groupings about which we are as yet entirely ignorant. The chemical molecule of iodine or of bromine—that is, the smallest quantity which can exist in the free state—is at high temperatures different from the chemical molecule at low temperatures.

The time will not permit me to dwell longer upon the chemical labours of Dumas. I should like to have said something concerning certain of his other researches in pure chemistry, since some of them, as, for example, his investigations on the nitriles, mark important points of departure. In his later years Dumas returned to many of the chemico-physiological problems with which, in association with Prévost, he began his career as an investigator. Some of these problems he carried forward into the new field of vegetal physiology, and traced many of the connecting links between animal and vegetable life. The question of the formation of fat within the animal has long troubled physiologists. The evidence afforded by chemistry was equivocal. Dumas and Boussingault maintained that the fat accumulating in the body of the animal was supplied pre-formed by plants. On the other hand, Liebig was of opinion that the animal had the power of converting carbohydrates, *e.g.* sugar and starch, into fat. Dumas himself supplied experimental confirmation of the validity of Liebig's opinion by his remarkable observations on the origin of bees'-wax. The nature of this substance was first revealed by the investigations of

the late Sir Benjamin Brodie, who had shown that it consists mainly of compounds of fatty acids belonging in fact to the same group of bodies as acetic acid itself. Dumas found that bees, even when fed exclusively upon sugar, still retained the power of producing wax, contrary to the opinion of Swammerdam and Maraldi, who were of opinion that the bee secreted the wax at the same time it extracted the honey from the flower.

The subject of fermentation has had special attractions for French chemists. The conditions of the transformation of sugar into alcohol under the influence of a ferment were the subject of one of Dumas's latest papers. Although he decides in favour of the physiological theory as opposed to the catalytic theory of Berzelius, or the eremacausis theory of Liebig, Dumas points out that alcoholic fermentation may be studied in the same way as any other chemical phenomenon; the conversion of sugar into alcohol and carbonic acid is a purely chemical phenomenon, but a chemical phenomenon caused by vital and not exclusively by chemical and physical forces.

Few scientific men have displayed the many-sidedness of Dumas, and any sketch of the man which failed to clearly bring out this feature would be wanting in biographical truth. I will briefly glance, therefore, at the characteristics of Dumas as a man of letters, as an educationist, and as a politician; in all three capacities he has influenced the intellectual and political development of his country.

Clearness of statement and elegance of diction are among the chief characteristics of French style, and in the exercise of these qualities no French scientific writer has surpassed Dumas. Much of his influence

among his countrymen is to be traced to the attractive and even graceful manner in which he sets before his readers what he has to say. The logical precision of his ideas apparently made composition an easy task to him. Your easy writing, said Byron, paraphrasing Sheridan's well-known saying, is usually damned hard reading; but no one could say this of Dumas. The nine volumes of his *Treatise on Chemistry applied to the Arts*, form a monument, not only to his industry and skill in compilation, but to his power of co-ordination, and to the excellence of his literary faculty. His literary power is even more strikingly evident in his "Lectures on Chemical Philosophy," delivered during the summer of 1836 in the Collége de France. It is not often that our lecture rooms resound with passages so pregnant with "fervency, freedom, fluency of thought," as may be gathered from the pages of this book. This, for example, is how he delineates the method of chemical inquiry — "And what is this method, which, old as our science itself, is still what it was in the days of its infancy? It is an absolute belief in the testimony of the senses, a boundless confidence in experiment, a blind submission to the supreme authority of facts. Chemists, ancient or modern, desire to see with the eyes of the body, before using the eyes of the mind; they think it right to make theories for facts established, but not to seek facts for theories preconceived."

The death of Lavoisier is probably the most tragic event in the history of science, and we can well imagine the thrill which must have passed through every auditor as Dumas, on the forty-second anniversary of that event, recalled the circumstances under which that brilliant career came to its untimely close. "It

was at the time that Lavoisier had conceived the idea of publishing a collection of his memoirs. . . . But whilst preparing this publication he was struck down by an atrocious death, and the collection remains incomplete, the most touching monument presented by the history of science. Nothing is more painful than to glance at this work, of which the second volume only is finished, whilst the first and third, in process of printing, appear to be cut off by the same axe which laid their author low. The sentence is broken off at the place where the pen stopped as the executioner laid hold of him. Nothing, I repeat, can possibly excite more acute emotion—nothing a deeper sense of the tragic element in the fate of man—than the sight of those doleful pages, the continuation of which is hidden by a veil of blood."

Years afterwards Dumas was the means of making France pay the debt of gratitude which the world owes to Lavoisier by completing, in truly imperial style, the edition of the works of the famous chemist. He first began to move in the matter in 1843, and at his instigation an influential committee was nominated by the Academy to inquire into the best manner of publishing the works of Lavoisier, but it was only after repeated applications to successive Ministers of Instruction that the necessary decree was obtained. The result was the publication, in a series of magnificent quarto volumes, superbly illustrated, of the memoirs and treatises of the great French master—a fitting monument of his imperishable genius.

Dumas's power as an orator was remarkably exemplified in the Faraday Lecture, which, on the invitation of the Council of the Chemical Society of London, he delivered in the theatre of the Royal Institution in

Albemarle Street, in the very lecture hall which is sacred to the memory and genius of the illustrious philosopher. It was the first of the series of addresses which the Society has instituted to commemorate the services of Faraday to science and to humanity. No one who had the good fortune to be present on that occasion can ever forget the manner in which Dumas discharged the duty which had been entrusted to him. The lecture hall at the Royal Institution has been the scene of many a rhetorical triumph, and its audiences have been held spellbound by some of the most glowing and eloquent periods which the literature of science has preserved for us, but they have listened to no truer or more beautiful words than those in which Dumas dwells upon the sentiments of respect and admiration with which civilised humanity regards the memory of Faraday. "The name of your illustrious fellow-countryman is not one which any single nation can claim as its exclusive property; his labours and discoveries are as widely recognised in France, in Germany, and in America, as in England. Faraday belongs to the whole world. There is not a spot on this earth, to which civilisation has penetrated, that does not claim the right of participating in the respect and gratitude you entertain for him. . . . Faraday is the type of the most fortunate and most accomplished of the learned men of our age. His hand, in the execution of his conceptions, kept pace with his mind in designing them; he never lacked boldness when he undertook an experiment, never lacked resources to ensure success, and was full of discretion in interpreting results. His hardihood, which never halted when once he had undertaken a task, and his wariness, which felt its way carefully in

adopting a received conclusion, will ever serve as models for the experimentalist."

Dumas's memorial addresses are indeed examples of their kind: it is impossible to know what to admire most in them—the grace of style, the lucidity of exposition, the wealth of illustration, the rare sympathy, or the marvellous faculty of presenting to us, by a few bold and graphic strokes, a striking and lifelike conception of the person commemorated. These addresses range over all subjects and over almost every department of human knowledge. The series opens with that on Bérard, and the estimate of the famous surgeon's relations to the science of his time might have been drawn up by a specialist. The same mastery of his subject is seen in the commemorative addresses on Geoffroy Saint-Hilaire, the anatomist, and on Auguste de la Rive, the physicist. In his address on the Brongniarts, Alexandre the geologist, and Adolphe the botanist, he shows with admirable grasp of detail the outcome of their scientific work in fields remote from his own special sphere, and his comparison between the labours of Cuvier and the elder Brongniart forms a chapter in the history of palæontology. Of peculiar interest to chemists are his monographs on Pelouze, who began life in the Rue Copeau, where he dined on bread and water, and had to open the window of his apartment in order to pull on his coat, and who ended it as Master of the Mint in a palatial residence on the Quai de Conti; and on Balard, the discoverer of the element bromine—a man remarkable for the Spartan simplicity of his habits and his supreme contempt for wealth and luxury. As Secretary of the Institute, Dumas in 1876 became a member of the French Academy, in succession to Fontenelle, Con-

dorcet, Fourier, Cuvier, and Flourens, his predecessors in office, and it fell to his duty to deliver the commemoration address on Guizot. The task was unusually difficult, but Dumas accomplished it with his never-failing tact and grace, and we have a vivid picture of the statesman, the man of letters, and the philosopher, who for so many years guided the destinies of France, and whose own career was but the reflex of much of his country's history.

Dumas's position in the Institute necessarily entailed upon him many onerous and conflicting duties, and endless is the series of reports in the *Comptes Rendus* in which he was concerned. Among the most recent, the best known are perhaps those relating to the diseases of the silkworm and to the action of the dreaded *Phylloxera vastatrix*. One of the great sources of the national prosperity of France seemed to lie at the mercy of this parasite. We learn from Dumas's report, presented to the Academy in 1873, that *Phylloxera*, up to that time unknown both to the cultivators of the vineyards and to naturalists, first made its appearance in the department of Gard in the year 1865. Its devastating action was confined to this region up to 1872, when it gradually spread over other wine-producing districts, and threatened the Gironde. The report traces the natural history of *Phylloxera*, so far as this is known, and shows that in the early stage of its development the pest is very susceptible to the action of numerous destructive agencies, such as phenol, decoctions of tobacco, quassia, etc. On the whole, however, the most generally efficient substances seem to be the salts known as the thiocarbonates, the employment of which was first suggested by Dumas.

No record of Dumas's literary services would be complete without some reference to his connection with the *Annales de Chimie et de Physique*. This journal, at one time the leading scientific periodical of the world — more renowned even than our own *Philosophical Transactions* — was instituted in 1790 by that band of scientific men of which Lavoisier was the dominant figure. In 1840 Dumas became one of the editors, and he remained an active member of the committee of publication down to the time of his death. He was thus for upwards of forty years connected with the responsible management of the journal, to which his own contributions extend over a period of half a century.

Probably no teacher in France has exercised so widespread an influence on the development of chemistry on French soil as Dumas. Public laboratories of the type which Germany first instituted were unknown in France until recently. That country owes their establishment to the example of Dumas. In 1832 he founded and supported a laboratory of research, entirely at his own expense, at the Polytechnic School, and afterwards at the Rue Cuvier. It was in these buildings, surrounded by a little knot of workers, comprising Piria, Stas, Melsens, Leblanc, and others, that those classical researches of which mention has been made were executed. The troubles of 1848 reacted disastrously upon the fortunes of Dumas, and he was unable to afford the heavy annual expense of a public laboratory, to which all students were admitted gratuitously; and for a time he was compelled to relinquish experimental teaching. Offers of assistance, however, were not wanting; and Dumas has himself related how, on a certain day during the troubled times which followed

the Revolution of 1848, he received a visit from a man of strange appearance and brusque demeanour. "They tell me," he said, "that you have shut up your laboratory; but you have no right to do so. If you are in want of money—there" (and he threw down a roll of bank notes); "take what you want, do not stint yourself; I am rich, a bachelor, and have but a short time to live."

Dumas felt himself unable to accept the proffered aid. Within a couple of years his visitor died, leaving 200,000 francs to the Academy of Sciences to found an annual prize for the promotion of research in organic chemistry. The Jecker Prize is now one of the most coveted distinctions of the Academy.

Dumas's career as a politician began with the Revolution of 1848, and he was sent by the electors of the arrondissement of Valenciennes to the newly-created Legislative Assembly. Shortly afterwards he was designated by the President of the Republic as Minister of Agriculture and Commerce, and was entrusted with the conduct of a number of enactments dealing with the social amelioration of the working classes. On the creation of the Empire, Dumas was made a senator, and he occupied this position down to 1870. During eighteen years' tenure of the office, scarcely a session passed without Dumas bringing his influence or his knowledge to bear upon some matter of social or economical importance. Questions of high policy he left to others; but on the subjects of coinage, or drainage, on trade marks, on the treaties of commerce between England and France, and on questions relating to education—when Dumas ascended the tribune it was to be listened to with the respect and interest which his knowledge and oratorical skill invariably commanded.

Shortly after his elevation to the Senate, Dumas was made a member of the Municipal Council of Paris, of which body he became Vice-President in 1855, and President in 1859, and he took a leading part in that wonderful transformation of the capital which is one of the chief glories of the Second Empire. Under his administration and direction Paris acquired her present systems of water-supply, lighting, and drainage.

The catastrophes of 1870 brought Dumas's career as a politician and administrator to a close, and he was henceforth able, like Guizot, undiscouraged and unembittered, to forget the minister in the man of science. The renunciation of his work as an investigator had brought many regrets with it; and near the close of his life he was heard to declare that throughout his career, full of triumphs as it was, he had nowhere felt the peace and happiness which his laboratory in the old days had afforded him. "My life," he wrote to one of his friends, "has been divided between the service of science and that of my country. I would rather have remained the servant of science alone; but, sprung from the obscure ranks of democracy, I have always thought that my country had done so much for me that no devotion on my part could be refused to it. If I have deceived myself, science itself will not hold me guilty. In limiting myself to scientific pursuits, I should have been happier, my life would have been a less anxious one, and perhaps I should have attained a larger view of truth."

One of the last of the more important public duties of Dumas was the inauguration of the French Association of Science—a body modelled on lines similar to those of our own British Association. How loyally the heart of "the old man eloquent" remained true to

science may be gathered from the following noble passage in the address he then delivered:—"Truth is beautiful enough in itself to merit a homage abstract and pure; the *rôle* of science noble enough to satisfy the aspirations of the most exalted intellects; its field vast enough to offer a harvest to every worker. Some cut down rich crops, others are content to glean; but that which each gathers or discovers all enjoy; among men of science goods are in common; and the torch, kindled by genius, is not extinguished, even when it has communicated, from place to place, its quickening flame to the entire world.

"Allow me to add that the recollections of an already long life have permitted me to become acquainted with a great variety of personages. And if I call on memory to picture to me how the type of true happiness is realised on earth, I do not see it under the form of the powerful man clothed in high authority, nor under that of the rich man to whom the splendours of luxury and the delicacies of well-being are granted, but under that of the man of science, who consecrates his life to penetrating the secrets of nature and to the discovery of new truths. Laplace, following out, for half a century, the application of the laws of the cosmic system to the movements of celestial bodies; Cuvier, inventing comparative anatomy and creating anew the ancient population of the globe; De Candolle, writing the elementary theory of botany and the description of all the known plants; Brongniart, showing how to classify soils by the fossils characterising them,—these illustrious men of science, and others, who, taking them as models, have done honour to your city, and whose names are on all your lips, have known a happy life. Animated by the love of truth, and indifferent to the

enjoyments of fortune, they have found their reward in public esteem."

Such outward and visible signs of appreciation as potentates and public bodies could bestow Dumas reaped to the full. Nearly every academy in Europe delighted to honour him. At the age of thirty-two he became a member of the Institute; Correspondent of the Berlin Academy in 1834; and a Foreign Member of the Royal Society of London in 1840. In 1843 the Royal Society bestowed upon him its highest distinction by the award of the Copley Medal, and he was the first recipient of the Faraday Medal from the Chemical Society of London. He was made a Knight of the Prussian Order *pour le mérite*, and he received the Grand Cross of the Legion of Honour.

Blessed with a singularly equable temperament and a vigorous constitution, and with a nature as warm and sunny as that of his native province, Dumas maintained his habits of "iron industry" to the last. At the beginning of 1884 he first showed indications of impaired physical powers; and, after a brief illness, he died at Cannes on the 11th of April, having nearly completed his eighty-fourth year. He was buried at Mont Parnasse. "Such a commanding figure cannot pass into forgetfulness. Your memory, Dumas, shall be perpetuated; your name transmitted from age to age. You will live in your works, in the example you have given, in the immortal productions and rare qualities of your mind: *Forma mentis æterna.*" And Wurtz — himself, alas! too near the bourn of that undiscovered country into which his great compatriot had passed — in uttering these words expressed the sentiment of the civilised world.

XII

HERMANN KOPP

An Address delivered to the Fellows of the Chemical Society, 20th February 1893 [1]

By the death of Hermann Kopp, a year ago to-day, the army of science lost one of its generals of division, and our own Society one of the oldest and most distinguished of the eminent company we are proud to call our Foreign Members. It is significant of Kopp's power and promise, and of the position that he so early won for himself in the commonwealth of science, that he should have been elected a Foreign Fellow of our Society so far back as 1849, when he was barely thirty-two years of age. With the exception of his illustrious colleague Bunsen, who is the *doyen* of the Forty, and whose jubilee as a Foreign Member we celebrated last year, and of the veteran Fresenius, who was elected in 1844, he was, at the time of his decease, the Senior Member on the list.

As the end of the century draws near, one after another of the men who have made this century what it is are passing over to the majority. It was characteristic of Kopp, in whom humour and pathos were happily blended, that he should have taken leave

[1] Chemical Society's Memorial Lectures, No. II.

of Hofmann, at what proved to be their last meeting, with the closing words of the song :—

> Der Herr im Himmel schenk ein gnädig End
> Uns letzen Zehn vom vierten Regiment.

It would seem as if, in the sunset of life, he had seen the shadow of a coming event. Both the friends have now submitted themselves to the strict arrest of the fell sergeant, and have gone

> to join
> The innumerable caravan which moves
> To that mysterious realm where each shall take
> His chamber in the silent halls of death.

The nineteenth century has witnessed an extraordinary expansion of that branch of science which it is the proper business and true function of this Society to foster. The history of learning can show no parallel to it. Contrast the condition of chemistry in the eighteenth century with its position to-day. It is true there were giants in those days: Black, Scheele, Cavendish, Priestley, Lavoisier, are men never to be forgotten. But their merit rests not more on the number or magnitude of their discoveries than on the influence they exercised on an intellectual movement, which it is at once the privilege and the glory of this century to have furthered and accelerated. Indeed we venture to think that the dispassionate historian of this movement, who, with "the cold neutrality of an impartial judge," weighs and assesses the part which successive workers have had in its progress, will recognise in the giants of our own time—men whose names are familiar in our mouths as household words—the signs of a mental stature not only as great as, but

possibly even greater than, that of the greatest of their predecessors.

The history of an epoch is the history of its leading men. They are the centres and sources of intellectual energy. In them the ever-widening waves of mental progress have their origin, and it is under their vivifying influence that science and learning grow and spread.

Hence, therefore, we do well, from time to time, to gauge our gain in knowledge by contemplating the life-work of the men who have fashioned it, and who have stamped it with the marks of their power and individuality. It is this consideration that has induced your Council to see in the occasions which the stern act of death compels us to notice, opportunities, not only of recording our sense of reverence, esteem, and admiration for those who have so faithfully tended the lamp of learning, but also of tracing the immediate outcome of their labours, and of measuring its effect on contemporary science.

By the wish of your Council I appear this evening, on this the first anniversary of the death of Kopp, to discharge what is to me a pious duty. Five-and-twenty years ago it was my good fortune as a student in Heidelberg to come into contact with Kopp, and to be influenced by his work and teaching.

To know Kopp was to love him, and to love him was, as Steele wrote of another, a liberal education. For to his friends he was ever ready to display the ample treasures of a mind rich not only in the lore of an ancient learning, but stored with the knowledge of a time of great achievement and of profound historical interest—the time which stretches from the closing years of Berzelius down to the final decade of the century;

which covers the period of the grand movement which had its inception in the little laboratory on the banks of the Lahn, and which witnessed those memorable intellectual combats between the champions of opposing schools of chemical thought, the echoes of which are only faintly heard, if heard at all, by the student of to-day. Kopp's colloquial powers were admirable; like the Great Lexicographer, he loved to fold his legs and have his talk out. His strong common sense; his vigorous, incisive thought: the range of his information of men and letters; his quick, retentive memory; his felicity of expression; his fund of anecdote; his ready wit and genial humour—all made him delightful to listen to. To watch the play of his features as he talked was in itself a recreation. Every line in the quaint, impressive face was instinct with intelligence, the outward and visible sign of the active, restless *geist* behind it. Not that there was any sense of unrest about the man. When I first knew him, a quarter of a century ago, he may be said to have passed the noontide of his intellectual career; he was living the quiet, contemplative, post-meridian life of a philosopher whose period of active service as a researcher was wellnigh spent, happy in his surroundings and in his occupation; his social sympathies satisfied by his relations with colleagues like Bunsen, Kirchhoff, Helmholtz, Renaud, Königsberger, and Zeller; and his mental activity finding scope for itself in his lectures, in the exercise of the various offices connected with the management of the University which he was called upon to fill, and in his literary labours. His life, like that of the greater number of men of science, had not been what the world calls eventful; the successive steps in his preferment as a

teacher and the appearance of his books and memoirs were the main incidents which marked the even tenor of his way. Born 30th October 1817, at Hanau, he was early attracted towards the study of natural science. His father, Johann Heinrich Kopp, a distinguished physician, seems to have occasionally occupied himself with experimental chemistry, and Leonhard's *Taschenbuch* and Gehlen's *Journal* contain papers by him on mineral analysis, and on investigations relating to physiological chemical products. The younger Kopp received his school training at the gymnasium of his native town, where he acquired that knowledge of Latin and Greek, and that love for classical learning which he turned to such signal account in the preparation of his great work on the History of Chemistry. When eighteen he proceeded to Heidelberg, to the University which eventually claimed him as Professor, where he studied chemistry under Leopold Gmelin, and physics under Wilhelm Muncke. Gmelin, who became Ordinary Professor of Medicine and Chemistry in the University in the year in which Kopp was born, is mainly known by his *Handbuch der Chemie*, of which an English translation by the late Henry Watts, the first editor of our Journal, was published by the Cavendish Society. Muncke is chiefly remembered on account of his share in the production of Gehler's *Physikalisches Wörterbuch*, and by his work on the thermal expansion of liquids. Wöhler has told us something of the conditions under which practical chemistry was studied in Heidelberg during the first quarter of the century, from which we may gather that Kopp could have had but few opportunities of gaining experience of manipulative work in the old cloisters which at that

time did duty as the University laboratory. He left Heidelberg for Marburg, where he graduated in 1838, presenting to the Philosophical Faculty as his thesis an essay entitled, "De oxydorum densitatis calculo reperiendae modo," from which it is evident that he had already, whilst barely twenty-one years of age, been attracted by those problems which were to constitute the chief experimental labours of his life. From Marburg he passed to Giessen, drawn thither by the influence which has made the Giessen laboratory famous in the annals of chemistry. At Liebig's instigation, and under his direction, he studied the mode of decomposition of mercaptan by nitric acid. This, which was for the most part a repetition of the work of Löwig and Weidmann on ethyl-sulphonic acid and its salts, is, practically, the only investigation in pure chemistry that Kopp ever published. Work of this kind had evidently few attractions for him, and he quickly returned to those studies which, as his inaugural dissertation shows, he felt that he was most fitted to pursue.

In 1841 Kopp became a *Privatdozent* in the Giessen University, lecturing alternately on theoretical chemistry, crystallography, meteorology, and physical geography. At about this time—that is, when twenty-four years of age—he began his *History of Chemistry*, and, as his material accumulated, he added this subject to the list of his lecture courses. In 1843 he became Extraordinary Professor, and on the removal of Liebig to Munich in 1852, he and Heinrich Will were together made Ordinary Professors, and were entrusted with the charge of the Giessen laboratory. But to a man like Kopp such a position was certain to prove uncongenial; he was not fitted, either by temperament or by training, to carry

on the traditions of a place so indissolubly associated with the name and fame and field of work of another, and after a year he resigned the sole control to his friend and colleague. Kopp remained in Giessen nearly five-and-twenty years, and all his most important experimental work was done there. In 1863 he received a call from Heidelberg, which he accepted; here, as has been said, he remained until his death, occupying himself latterly with lectures on the history of chemistry and on chemical crystallography. He was repeatedly solicited to accept a position in some one of the larger universities, notably in Leipsic and in Berlin; but all attempts to draw him from his dear Ruperto-Carolina were unavailing. "Even Bunsen alone," he was wont to say, "keeps me fast in Heidelberg." And by no one is Kopp's departure more keenly felt than by Bunsen, his friend and colleague for more than a quarter of a century. The strollers on the Anlage still miss the quaint figure on its way to the daily visit to the old veteran, who, rich in honour and in years, is now the last of that famous group which has made Heidelberg renowned as a centre of intellectual life and scientific activity.

Kopp is best known to the literary world by his *History of Chemistry*. The first volume of this work, a monument of learning and of patient labour, of constructive skill and sagacious criticism, appeared in 1843, and the fourth and final volume in 1847. His life-long friend Hofmann, who was with him at Giessen, has told us that, by the publication of this classical work, Kopp, then barely thirty years old, suddenly found himself famous. "With one accord his contemporaries recognised that here was a production which, whether they regarded the thoroughness of research

that it displayed, or the manner in which the material resulting from that research was sifted and arranged, was without a parallel in the literature of any other country. And even to-day, after the lapse of nearly half a century, there is no historical work on chemistry that can be even remotely compared with it. Numbers of books relating to the same subject, some of considerable merit, have since been published in Germany and France, but it is not difficult to perceive that they are all grounded on Kopp's great work." For upwards of forty years Kopp had it in contemplation to bring out a new edition, and much of the later historical work he published, such as his *Beiträge zur Geschichte der Chemie*, which appeared between 1869 and 1875, and the *Entwicklung der Chemie in der neueren Zeit*, printed under the auspices of the Historical Commission of the Bavarian Academy in 1873, together with the two volumes on *Die Alchemie in älterer und neuerer Zeit*, grew out of the materials he had gathered together. "But," again to quote Hofmann, "the better is here the enemy of the good. Kopp postponed the 'vermehrte und verbesserte Auflage' year after year, in the hope of being able to make a fuller study of certain special periods. Whoever is familiar with the mass of profoundly interesting matter he had accumulated, or who has had the opportunity of seeing the bulky note-books in which it was stored, must deeply lament that the hand which could alone arrange these treasures is now stiffened in death."

On the death of Berzelius in 1848 the leaders of the Giessen school determined to carry on the work which had chiefly occupied the closing years of the Swedish chemist. Berzelius's *Year Book* had become a power in the chemical world, mainly on account of the authority

wielded by the greatest chemical critic of his time. The *Jahresbericht* of Liebig and Kopp differed, however, fundamentally, both in plan and execution, from its Swedish prototype. It was to be a review of the year's progress, not only in chemistry, but also in all those sciences which were associated with chemistry, or were, in any definite sense, ancillary to it; it was to be done impartially, and with no special reference to any set of dogmas or particular school of chemical thought. Practically the whole of the more active members of the scientific side of the Philosophical Faculty of the University were concerned in its production. To Kopp fell the greater share of the arrangement and of the general editorial management; in addition, he undertook the summaries relating to Theoretical, Physical, and Inorganic Chemistry. To Buff and Zamminer were entrusted Pure Physics; to Heinrich Will, Organic Chemistry; to Knapp, Technical Chemistry; to Ettling, Mineralogy; and to Dieffenbach, Chemical Geology. The first volume appeared towards the close of 1849, and consisted of a review of the work of 1847 and 1848. Liebig continued to be associated with Kopp as editor for some years after he left Giessen, but in 1857 his place was taken by Will, who acted as co-editor until 1862, when Kopp resigned his share in the responsible direction of the publication prior to his removal to Heidelberg. No chemist active in the prosecution of research needs to be reminded of the value of the *Jahresbericht*. It has undoubtedly exercised a most beneficent influence on the development of chemical science in Germany, and it has been of the greatest service to those chemists in this country to whom German is not an unknown tongue. The attempt was made to introduce the *Jahresbericht* to English readers

by means of a translation published under the direction of Hofmann and Warren De la Rue, but the good seed fell upon stony places, and after a year or two the enterprise was abandoned. Matters in 1849 were doubtless better with us than Liebig found them to be in 1837, when he told Berzelius that England was not the land of science, and that our chemists were ashamed to call themselves so because the apothecaries had appropriated the name.[1] Possibly the reason for the ill success of the venture is to be found in the fact that the Journals of this Society have, from the outset, taken note, in a more or less systematic way, of current chemical work in other countries. Be that as it may, there is practically no longer room for the German publication in this country ; since 1871 our Society has taken in hand an English *Jahresbericht*, and issues to its members monthly abstracts of all papers relating to, or bordering on, chemistry, in which, as regards fulness, comprehensiveness, and promptitude of publication, we have striven to emulate the example, and possibly even bettered the instruction, of our distinguished Foreign Member.

In 1851 Kopp joined Liebig and Wöhler in the production of the *Annalen der Chemie und Pharmacie*, and for many years he took the responsible share in its management. His name as editor appears on the title-page of no fewer than 190 volumes of this famous

[1] "Ich bin einige Monate in England gewesen, habe ungeheuer viel gesehen und wenig gelernt. England ist nicht das Land der Wissenschaft, es existirt dorten nur ein weitgetriebener Dilettantismus, die Chemiker schämen sich Chemiker zu heissen, weil die Apotheker, welche verachtet sind, diesen Namen an sich gezogen haben. Mit dem Volke war ich ausserordentlich zufrieden, Zuvorkommenheit, Gastfreiheit, kurz ich habe sonst an ihnen alle Tugenden gefunden. Graham macht auch in wissenschaftlicher Hinsicht die schätzbarste Ausnahme, er ist ein vortrefflicher Mensch, auch Gregory, der an seine Stelle in Glasgow gekommen ist."—Liebig to Berzelius, 26th November 1837.

periodical, which, under its present designation of Justus Liebig's *Annalen der Chemie*, constitutes an abiding monument to the influence and power of its great progenitor.

Kopp's services to the literature of our science were, however, by no means confined to its journalism. Engrossing and arduous as his duties as an editor must have been, he yet found time to write the admirable *Introduction to Crystallography*, and to prepare the section on "Theoretical Chemistry" in that well-known text-book, Graham-Otto's *Lehrbuch der Chemie*. These works enjoyed great popularity in Germany, and have exercised no inconsiderable influence on the education of the present generation of chemists in that country. The *Introduction to Crystallography* was indeed written specially for chemists with a view of interesting them in a study which is still too frequently neglected by them; and the "Theoretical Chemistry" proved of signal service in disseminating the doctrines which constituted the "new chemistry" of thirty years ago.

No record of Kopp's literary activity would be complete without a reference to his occasional writings, some of which are among the most typical and most characteristic of his productions; as, for example, his *Aus der Molecularwelt*, written for Bunsen's seventieth birthday, and the *Aurea catena Homeri*, with which he greeted Wöhler when fourscore years of age. In the *Molecularwelt*, Kopp's delicate fancy and quaint humour are seen at their best; the book attracted considerable attention even beyond chemical circles, and rapidly ran through a number of editions. His lecture *Sonst und Jetzt in der Chemie*, which appeared in 1867, may still be read with interest as an historical account of the changes which have conduced to the present development

of chemical theory. His pen, indeed, in spite of frequent illness and waning strength, was busy to the last, and he closed a long half-century of literary labour in preparing for Ostwald's "Chemical Classics" an annotated edition of Liebig and Wöhler's famous memoir on the radicle of benzoic acid—the memoir which, it will be remembered, Berzelius hailed as the dawn of a new day.

Kopp's scientific papers relating to his experimental and critical labours appeared mainly in Poggendorff's *Annalen* and in the *Annalen der Chemie und Pharmacie*. Two or three of his early communications were printed in the *Philosophical Magazine*, and his elaborate memoir on the specific heat of compound substances, in which he sought to develop Neumann's law, was published by the Royal Society.

Compared with those of his great contemporaries, Liebig and Wöhler, the number of his scientific papers may seem small. But, in estimating Kopp's industry and power of work as an investigator, the character of his experimental researches must be considered. The nature of the relations which he strove to elucidate often necessitated the determination of the physical constants of some scores of substances, many of which could only be prepared in a state of sufficient purity by lengthy operations. Moreover, these innumerable measurements were not only troublesome and tedious in themselves, but they were followed, very frequently, by long and wearisome calculations. The rich field of chemical physics will indeed only yield its fruit by patient and incessant tillage.

Kopp occupies an almost unique position as an investigator. The one consistent purpose of his work was to establish a connection between the physical

and chemical nature of substances; to prove, in fact, that all physical constants are to be regarded as functions of the chemical nature of molecules. It is not implied, of course, that the conception of such an intradependence originated with him. As a matter of fact, almost immediately after the publication of Dalton's *New System of Chemical Philosophy*, in which the doctrine of atoms was revived to account for the fundamental facts of chemical union, the endeavour was made to connect the chemical attributes of a substance with one of its best-defined physical constants, viz. its atomic mass. Prout's hypothesis is, in reality, the generalised expression of such an attempt; it is an adumbration of Mendeleeff's great discovery of the Law of Periodicity. But it may be justly claimed for Kopp that no one before him made any systematic effort to connect such of the physical qualities of substances as admit of quantitative statement with the stoichiometrical values of such bodies. The sporadic attempts made prior to 1840 were practically fruitless on account of the imperfect nature of the physical data up to that time extant.

When Kopp began his inquiries, very few boiling points were known, even approximately; and he had, as a preliminary step, to ascertain the conditions under which such observations must be made in order that accurate and comparable results could be obtained. The thermal expansions of barely half a dozen liquids had been measured, and the very methods of making such measurements with precision had to be worked out.

At the outset of his investigations, Kopp found the physical constants with which he was more immediately concerned very much as Berzelius found Dalton's values of the relative weights of the atoms; at the close of

his work they were hardly less accurately known than were those stoichiometric numbers to the ascertainment of which the great Swedish chemist had dedicated his life.

It would be quite impossible within the limits of such an address as this to attempt to give an analysis of each of Kopp's memoirs. I must, therefore, content myself with essaying to lay before you a generalised statement of the main outcome of his work, and of the development which it has received at the hands of his successors. Fortunately for the purpose of such a summary, Kopp's more important memoirs readily and naturally fall into comparatively few groups, viz. (1) those concerning the relations between the specific gravities of substances and their molecular weights; (2) those treating of the relations between boiling point and chemical composition; and (3) the papers relating to the specific heats of solids and liquids. As regards the other papers, only the briefest notice is here possible.

In the paper on Solubility (*Annalen*, xxxiv. 260 (1840)), Kopp seeks to determine the behaviour of a solvent towards a mixture, in excess, of two soluble salts, or, in other words, to ascertain how far the solubility of one substance is affected by the presence of another when the conditions are such that chemical change is *a priori* impossible. Are the salts, he asks, dissolved in the ratios of their specific solubilities; or is the solubility of the one salt unaffected by the presence of the other; or can the aggregate amount of the dissolved saline matter be deduced from the specific solubilities of the constituents? He finds, as regards the solubility of two salts possessing a common basic radicle, that they are never present in the saturated solution in the

exact amount required by their specific solubilities. In the case of two salts containing an acid radicle in common, it was observed that the salt having the "stronger" base preserved its specific solubility, and that the other salt dissolved in the solution to an extent depending on its particular character, the quantity being sometimes greater and sometimes less than the calculated amount. He also shows how such determinations afford a method of recognising the existence of double salts in solution, but which, as in the well-known case of carnallite, are more or less decomposed on crystallisation. Lastly, he points out how the temperature corresponding to the change of hydration, or some other condition, in a salt, either in the dissolved or undissolved portion—as, for example, in the case of sodium sulphate—may be deduced from the formulæ expressing the relation of solubility to temperature, where, as in that salt, we have to do with two solubility curves.

This investigation is noteworthy as being the precursor of the extensive inquiries which we owe to Karsten, Hauer, Rüdorff, and Engel. In the note on the Solubility of Common Salt in aqueous Alcohol of different strengths (*Annalen*, xl. 266), Kopp opens up a field of labour which, twenty years later, taxed the energies of Schiff and Gerardin. No general law as to the partition of a soluble substance between two solvents, or as to the action of a mixture of two mutually miscible liquids in one of which the substance is insoluble, has hitherto been traced.

In the short paper on the Cohesion of Liquids (*Annalen*, xxxv. 230), he describes a simple method for the quantitative estimation of the resistance which liquids offer to rupture. The power which a liquid

possesses of resisting any tendency to internal separation of its parts has hitherto received but scant attention from physicists, presumably on account of the experimental difficulty of the problem. It is, as shown by Donny, Osborne Reynolds, and Worthington, more considerable than we are apt to imagine, and is indeed comparable with that of solids. It would be interesting to submit Kopp's method to a careful experimental criticism, for it is so easy of execution that, if found accurate, valuable data could rapidly be accumulated by its aid.

In the paper "Über die Bildung von Krystallen mit Kernen" (*Annalen*, xciv. 118 (1855)), Kopp describes a characteristically ingenious method of throwing light upon the mode in which natural crystals are made to enclose foreign substances, as in the case of fluorspar, which frequently surrounds cubes of iron pyrites, and where, too, as is obvious from the nature of the cleavage, no very firm adhesion exists between the substances. The plan consisted in coating the crystal to be enclosed with a thin film of coloured collodion and then placing it in a solution of the salt which was to surround it. In spite of Kopp's interest in chemical crystallography, which under his direction became a most fascinating study, he only published a couple of notes on the subject; in the first he treats of the pleomorphism of magnesium sulphate, and in the second he discusses the bearing of analogous atomic composition on crystalline form (*Annalen*, cxxv. 369 (1863)).

In nothing was Kopp's originality or ingenuity more strikingly manifested than in the construction of his apparatus. To a great extent he was his own instrument maker, and his materials were, for the most part,

glass and cork. But no Japanese worker, with his few and primitive tools, ever produced results which, in point of delicacy, finish, and accuracy, surpassed those which Kopp obtained by means of his simple contrivances. It is told of Wollaston that when a distinguished foreigner called to see his laboratory, he had his chemical utensils brought in on a tray. "This," said he, "is all the laboratory I possess." I venture to say that the whole of the apparatus which Kopp employed in the course of his researches on physical chemistry could be put on a very much smaller tray than must have been needed to hold the furniture of Wollaston's laboratory.

Kopp's first contribution to the literature of science consisted of an account in Poggendorff's *Annalen*, done when he was a student at Marburg, of a differential barometer which he had constructed (Poggendorff's *Annalen*, xl. 62 (1837)). His volumenometer (*Annalen*, xxxv. 17 (1840)) is an improvement on the stereometers of Say, Leslie, and W. H. Miller, not so much in simplicity of construction or of use, as in the greater convenience of its form, and in the more accurate results it is capable of affording. It may be readily made with materials ordinarily found in laboratories, and is especially useful for the determination of the specific gravities of such substances as sugar, starch, silk, cotton, wool, flax, etc.

His ingenuity is further seen in his method (*Annalen*, lxxxi. 1 (1852)) of determining the thermal expansion of substances which, like sulphur, galena, zinc, blende, fluorspar, etc., can only be obtained in irregular fragments, and frequently of no great size. Much of our knowledge respecting the thermal expansion of minerals is in fact based on Kopp's work.

Closely related to this memoir is that on the change of volume which substances experience in passing from the solid to the liquid state (*Annalen*, xciv. 129 (1855)). With the exception of the work of Réaumur, in 1726, which was only qualitative in character, practically nothing was known on this subject prior to Kopp's investigation. By means of an apparatus which is simplicity itself, and which could be put together in a few moments, Kopp determined the relation of the volume, when solid to that when liquid, of a large number of substances, such as ice, phosphorus, sulphur, stearic acid, calcium chloride, sodium thiosulphate, etc. The value of the apparatus lay, however, not so much in its simplicity as in the skilful work put into it, and in the exactitude of the results which came out of it. Although subsequent workers have had occasion to repeat Kopp's observations by other and far more elaborate apparatus, as, for example, that of Pettersson, the substantial accuracy of his observations has not been challenged. The main conclusions which may be drawn from his work are now the common property of the text-books. As a rule, the expansion coefficient of a solid substance increases very rapidly in the neighbourhood of the melting point, the most striking exceptions being phosphorus and ice. The increase in volume which a substance experiences in passing from the solid to the liquid condition depends, for the most part, on this rapid increase in the expansion coefficient and on the sudden expansion at the moment of melting. In some substances, like phosphorus, the change in the expansion coefficient is small, but the increase due to liquefaction is comparatively great; in wax, on the other hand, the increase in volume is chiefly due to

the change in the coefficient of expansion. Kopp was particularly attracted by the extraordinary behaviour of Rose's fusible metal, which had already been the subject of a careful examination by Erman in 1827. This alloy—a mixture of 2 parts of bismuth, 1 part of lead, and 1 part of tin—expands on warming from 0° to 59° in the ratio of 1 to 1·0027, but on further heating contracts, until at 82° it occupies the same volume as at 0°; it continues to contract until 95°, when its relative volume is 0·9947; it melts between 95° and 98°, when its volume suddenly becomes 1·0101. The nature of alloys had always a special interest for Kopp, and two of his shorter papers, "Ungleiche Mischung von Metall-Legirungen," etc. (*Annalen*, xl. 184 (1841)), and "Dichtigkeit des Cadmium-Amalgams" (*Annalen*, xl. 186 (1841)), had reference to this subject.

Kopp, as an investigator, will be mainly known hereafter by his attempts to connect the physical constants of substances with their molecular weights and chemical composition. The greater number of his memoirs indeed are concerned with these relations. As already stated, his earliest paper on the subject was the dissertation he presented for his doctorate to the Philosophical Faculty of the University of Marburg.

I shall first treat of his work on the specific heats of bodies; then of his attempts to connect the boiling points of liquids with their chemical nature; and lastly, of his long-continued efforts to trace the relations between the volumes and molecular weights of substances.

In 1818 Dulong and Petit found that when the atomic weights of the elements are multiplied by their respective specific heats approximately the same

number is obtained. This fact was subsequently embodied by them in the statement that "the atoms of all simple bodies have exactly the same capacity for heat." In 1831 this generalisation was extended by Neumann (Poggendorff's *Annalen*, xxiii. 1) so as to include compounds, and what is now known as Neumann's law was thus expressed by its discoverer: "In bodies of analogous chemical composition the specific heats are inversely as the stoichiometrical quantities, or what is the same, stoichiometrical quantities of bodies of analogous chemical composition have the same capacity for heat."

Subsequent observers, among whom may be named Avogadro, Hermann, and especially Regnault, Person, and Pape, added greatly to the experimental material on which the validity of Neumann's law is founded. The whole question was, however, reopened by Kopp in 1864. By means of a simple modification of the method of mixtures, Kopp determined the specific heats of a large number of substances, for the most part compound in their nature. He concluded that each solid substance, at a sufficient distance from its melting point, has a specific heat which may vary somewhat with physical conditions (temperature, greater or less density, amorphous or crystalline nature, etc.); but the differences are never so great as must be the case if a variation in the specific heat of a substance is to be held as a reason for explaining why the determinations of the specific heats of solid elements do not even approximately obey Dulong and Petit's law, nor those of solid compounds of analogous chemical constitution Neumann's law. Neither law is universally valid, although Kopp found that Neumann's law applies in the case of many compounds of analogous composition

to which, on account of their totally different chemical deportment, different formulæ are assigned; and even in cases in which these laws have hitherto been considered as essentially true, the divergences from them are material. Each element has the same specific heat in its solid free state as in its solid compounds. From the specific heats to be assigned to the elements, either directly from experimental determination, or indirectly by calculation on the basis of the law just stated, the specific heats of their compounds may be calculated.

Kopp concludes his memoir with certain suggestive considerations as to the nature of the chemical elements. What substances, he asks, are to be regarded as chemical elements? Does the mere fact that they are non-decomposable determine this? Or may a body be non-decomposable in point of fact, and yet from reasons of analogy be regarded not as an element but as a compound? He reminds us that the history of chemistry furnishes numerous examples of cases in which sometimes one and sometimes another mode of view led to results which at present are regarded as accurate. The earths were non-decomposable in point of fact in 1789, when Lavoisier expressed the opinion that they were compounds, oxides of unknown metals. Lavoisier's argument was based on the fact that the earths enter as bases into salts, and that it was to be assumed in regard to all salts that they contained an oxygen acid and an oxygen base. But the view, founded on the same basis, that common salt contains oxygen, and the subsequent view that what is now called chlorine contained a further quantity of oxygen besides the elements of an oxygen acid, did not find an equally permanent recognition. On the basis of the

actual non-decomposability of chlorine, Davy, about 1810, maintained its elementary character; and this view became general when Berzelius adopted it—more, Kopp thinks, because he was outvoted than because he was convinced.

It cannot be maintained that the bodies which chemists regard as elements are absolutely simple substances. How far a body is to be regarded as an element is so far relative that it depends on the development of the means of decomposition which practical chemistry has at its disposal, and on the trustworthiness of the conclusions which theoretical chemistry can deduce. A discussion as to whether chlorine or iodine is an elementary substance can be taken only in the sense whether chlorine is as simple a body as oxygen or manganese, or nitrogen; or whether it is a compound body, as peroxide of manganese or peroxide of hydrogen, for example.

If Dulong and Petit's law were universally valid, it could be used as a test for the elementary nature of a substance whose atomic weight was known. That iodine, from a direct determination, and chlorine, by an indirect determination of specific heat, had atomic heats agreeing with Dulong and Petit's law would be a proof that iodine and chlorine, if compounds at all, are not more so than other so-called elements for which this law is regarded as valid.

According to Neumann's law, compounds of analogous molecular composition have approximately equal molecular heats. In general, compounds whose molecule consists of a large number of undecomposable atoms, or is of more complex constitution, have greater molecular heats; especially in those compounds all of whose elements follow Dulong and Petit's law,

is the magnitude of the molecular heat a measure of the complexity. If Dulong and Petit's law were universally valid, it might be concluded with great certainty that the so-called elements, if they are really compounds of unknown simple substances, are compounds of the same order. If we start from the elements at present assumed in chemistry, we must rather admit that the magnitude of the molecular heat of a body depends not only on the number of elementary atoms contained in one molecule of it, or on the complexity of its composition, but also on the atomic heat of the elementary atoms entering into its composition. It is possible that a decomposable body may have the same atomic heat as a non-decomposable one.

In a very great number of compounds the molecular heat gives more or less accurately a measure of the complexity of their composition, and this is also the case with those compounds which, from their chemical deportment, are comparable with the undecomposed bodies.

If ammonium or cyanogen had not been decomposed, or could not be so by the chemical means at present available, the greater molecular heats of their compounds, compared with those of analogous chlorine or potassium compounds, and the greater molecular heats of ammonium and cyanogen obtained by indirect determination, as compared with those of potassium and chlorine, would indicate the compound nature of these so-called compound radicles. The conclusion appears legitimate that, for the so-called elements, the directly or indirectly determined atomic heats are a measure of the complexity of their composition (*Philosophical Transactions*, 1865, 155).

These conclusions are, it should be stated, largely based upon the fact of the abnormally low atomic heats of bodies of low atomic weight, such as carbon, silicon, boron, glucinum, etc., when taken at comparatively low temperatures. Kopp would regard bodies of low atomic weight as of a low order of complexity. But since it has been recognised that the specific heat of such bodies is very largely affected by temperature, and in such manner that their atomic heats at high temperatures more and more closely approximate to the value required by Dulong and Petit's law, the force of Kopp's argument is materially weakened.

In 1841 Kopp, from observations on methyl and ethyl derivatives, drew attention to the fact that in many analogous chemical compounds the same difference in composition frequently occasions the same difference in boiling point (*Annalen*, xli. 87 *et seq.*). He also pointed out that in many other similar compounds, mostly alcohols, acids, and esters of the fatty series, a difference of $x\mathrm{CH_2}$ apparently corresponds with a difference of about $x19^\circ$ in the boiling point; that an acid boils 40° higher than the corresponding alcohol; and that an ester boils 82° lower than the isomeric acid. On comparing analogous compounds differing only in the number of C atoms, he further concluded that the boiling point of the compound having xC atoms more than the other was $x \times 29^\circ$ higher, and that a compound having xH atoms more than the other boiled $x \times 5^\circ$ lower. He subsequently found that this difference of 19°, corresponding with an increment of $\mathrm{CH_2}$, is not observed in all homologous series. Thus, in the benzene hydrocarbons $x\mathrm{CH_2} = x24^\circ$; in the fatty ketones and aldehydes it is 22°; in the alkyl chlorides it is 31°; and in the fatty anhydrides

it is 12·5°. Isomeric compounds of the same type and of the same chemical character have the same boiling point; if of the same type but of different character, or if of different type, their boiling points are different. Hence Kopp concluded that the boiling point of a liquid was a function partly of molecular weight and partly of chemical constitution. The influence of molecular weight alone may be studied by comparing bodies of strictly analogous constitution, *e.g.* the normal fatty acids. If Kopp's main contention is valid, the relations should hold good at other temperatures than that of the boiling point under atmospheric pressure, and by observations on the vapour pressures of the normal fatty acids, Landolt concluded that such was the case.

Further and more accurate observations showed however, that Kopp's original propositions must be materially modified. Wanklyn found that the increment for CH_2 in an homologous series became less and less as the series was ascended. Schorlemmer (*Philosophical Transactions*, clxxii. 123 (1872)) observed that in the homologues of marsh gas the difference corresponding to CH_2 appears to become steadily less by 4° until it reaches the value 19°. Linnemann (*Annalen*, clxii. 39), by means of his well-known fractionating apparatus, determined with great accuracy a number of boiling points of members of correlated groups of fatty compounds, from which he concluded that equal differences in chemical composition do not give rise to equal differences in boiling point, but that the differences diminish, at least in the lower members of the greater number of the series, as the series is ascended. Series, however, are known, as for example the alcohols, in which the differences for CH_2 are

approximately constant; in some, too, the differences increase as the series is ascended. Schumann, in 1881, studied the vapour pressures of esters of the fatty acids, from which he deduced conclusions in harmony with those of Landolt on the fatty acids in so far as they showed that the relations obtained were independent within certain limits of the magnitude of the pressure. At the same time, the relations themselves lent no support to Kopp's general conclusions.

The observations on boiling points which have accumulated since the time of Linnemann have confirmed the conclusions put forward by him. In homologous series the boiling point differences may either increase or decrease, or may vary irregularly as the series is ascended. Although in all cases it is not yet possible to assign causes for these apparent irregularities, they may to some extent be explained by considerations recently advanced by Dobriner and extended by Lossen.

It is possible to regard any three consecutive homologues containing carbon, hydrogen, and oxygen as methyl, ethyl, and propyl compounds. For example, the first three members of the series of normal fatty alcohols may be written—

MeOH,
EtOH,
PrOH.

Formulating the compounds in this way, Dobriner would term them a triad of oxygen compounds of the alkyls, for in each case the alkyl group is linked to oxygen. If, however, ethyl alcohol be taken as the initial member of the triad, the formulæ of ethyl, propyl, and butyl alcohols would be

$MeCH_2OH$,
$EtCH_2OH$,
$PrCH_2OH$,

and as here the methyl, ethyl, and propyl groups are linked to carbon, the triad would be spoken of as one of "carbon compounds" of the alkyls. Calling the difference between the boiling points of the methyl and ethyl compounds of any triad the "first boiling point difference," and that between the boiling points of the ethyl and propyl compounds the "second boiling point" difference, Dobriner deduced the following two rules:—In any triad of oxygen compounds the first difference is less than the second. In any triad of carbon compounds the first difference is greater than the second. These conclusions are in harmony with the exceptions to regularity frequently observed in the lowest members of homologous series, and the general applicability of the rules has been tested by Lossen. Lossen finds that, so far as trustworthy data go, triads of oxygen compounds almost invariably conform to the rule; in the case of carbon compounds, however, although in most cases Dobriner's rule is obeyed, there are exceptions, notably in the case of the alcohols, where the boiling points have been often and carefully determined.

In triads containing the alkyl groups linked to sulphur, nitrogen, the halogens, and the metals, Lossen finds that in some cases the first difference is less, whilst in others it is greater, than the second difference.

As regards the boiling points of isomers, subsequent work has proved that in no case are the boiling points identical.

In 1868 Hinrichs stated that the more symmetrical

the formula of an isomeric molecule is, the lower is the boiling point. Neumann extended this idea, and in 1874 put forward for paraffin derivatives the only rules which are capable of anything like general application. His conclusions are — That the more nearly the grouping of the atoms in an isomeric molecule deviates from the rectilinear or chain type, and approaches the spherical type, the lower is the boiling point. In metameric bodies of similar chemical character and otherwise corresponding structure containing carbon, hydrogen, and oxygen, the more nearly the oxygen atom stands to the middle of the chain of atoms the lower is the boiling point. These two rules applied singly or in conjunction accord with the great majority of the facts. There are exceptions, however, among which some of the esters carefully examined by Gartenmeister may be noticed.

For isomers of aromatic series practically no generalisations have as yet been established.

Several attempts have been made to deduce formulæ whereby boiling points may be calculated. In the case of homologues, expressions such as those of Goldstein, Mills, and Hinrichs are no doubt successful in reproducing the observations for the particular series to which the formulæ refer, but they do not afford a basis for calculating boiling points in general.

Before entering upon an examination of Kopp's memoirs on specific volume, it may be interesting to give a brief history of the attempts made prior to his time to trace relations between chemical combination and the equivalent volumes of solid and liquid substances.[1]

[1] Objection has been raised to the term "specific volume" on the ground that as specific gravity is the weight of unit volume, specific volume should,

These attempts may be said to have grown out of Gay Lussac's memorable discovery in 1808, that gaseous bodies combine together in simple volumetric proportions. The first endeavour to apply similar considerations to solids and liquids was made in 1821 by "Le Royer, Pharmacien, and J. A. Dumas, son élève," the latter of whom is none other than our eminent Foreign Fellow, and the first of our Faraday Lecturers, the late Perpetual Secretary of the French Academy. In their paper in the *Journal de Physique*, xcii. 408, Le Royer and Dumas attempted to determine the equivalent volumes of the elements by dividing their atomic weights by their respective specific gravities, the values so obtained being termed by them *atomic volumes*. They were led to conclude that these volumes formed an arithmetical series, a supposition which, it must be admitted, was hardly warranted by the facts then known, and which has since been completely disproved by more accurate observation. This idea of combination among solids in definite volumetric proportion was further developed in 1824 by W. Herapath in a paper

by analogy, be the volume of unit weight—the reciprocal of specific gravity, and not the product of this reciprocal and molecular weight. Hence, of late years, the term "molecular" volume has been more generally preferred. It is, however, questionable whether the change is worth making, in view of the confusion which is almost certain to occur from the varying sense in which the term will be used by physicists and chemists. The real molecular volume is obviously the mass of a molecule divided by its density. Although such values are not yet known with any degree of precision, it can hardly be doubted that we are within measurable distance of this knowledge. The term specific volume is certainly not free from objection, but inasmuch as it has acquired, by definition, a distinctive meaning, no difficulty need be apprehended from its continued use.

Specific gravity referred to water at 4° is the weight in grams of the unit volume. If, then, the molecular weight be expressed in grams, we may define specific volume as the number of cubic centimetres occupied by this weight. The molecular weights here used refer to the gaseous state: the objection which has been urged against the employment of such weights has, however, lost much of its force, now that the conception of the continuity of the gaseous and liquid states is generally accepted.

read before the Bristol Society of Inquirers (*Philosophical Magazine*, November 1824), in which it was sought to prove that the volume of the oxygen in a metallic oxide bears a simple ratio to that of the metal with which it is combined. Almost simultaneously the same problem was attacked by Karsten (*Schweigg. Journal*, lxv. 394), and subsequently in 1830 by Boullay, but with no definite theoretical result. Ammermüller, however, in 1840, was led to conclude (Poggendorff's *Annalen*, xlix. 341; *id.* l. 406), that the specific volumes of compounds containing the same elements in different proportions are either identical or stand to one another in rational proportions. Persoz, in his *Introduction to the Study of Molecular Chemistry*, recognised that equivalent amounts of many bodies of analogous composition occupy the same volume, and he inferred that the specific volumes of all substances are multiples of one and the same number, a conclusion also drawn by Le Royer and Dumas, but which is not supported by facts.

Kopp's first memoir on the subject of specific volume appeared in Poggendorff's *Annalen* for 1839 under the title "Ueber die Vorausbestimmung des specifischen Gewichts einiger Klassen chemischer Verbindungen" (Poggendorff's *Annalen*, xlvii. 133). In this paper he discusses the specific gravities of a number of compounds of metals and non-metals, and by means of certain assumptions he deduces general formulæ from which he is able to calculate the specific gravities of certain oxides and haloïd salts with results which show, in general, a fair agreement with the observed values. In the same manner he calculates formulæ for other anhydrous salts, such as sulphates, carbonates, and nitrates, on the supposition that such salts consist of

combinations of oxides and acids, or that they are made up of a radicle, acid, *plus* oxygen. By means of these formulæ he infers that it is possible to draw conclusions concerning the specific gravity of metals for which this constant is unknown. Kopp here uses the term *specific volume* for the first time, and he defines it as the molecular weight (*Mischungsgewicht*) of a body divided by its specific gravity. He finds that the specific volumes of similarly reactive elements, as, for example, chlorine, bromine, and iodine; tungsten, molybdenum, chromium, iron, manganese, nickel, cobalt, etc., are respectively equal or nearly equal. In other cases, as silver and gold, potassium and sodium, the specific volumes stand to each other in simple relations. Elements which, like barium and strontium, form isomorphous compounds show identity in specific volume, and Kopp sees in this fact an argument for the validity of the theory of hydrogen acids; for, if we conceive that baryta replaces the strontia in an isomorphous strontium salt, it follows that the baryta will occupy a wholly different volume from the strontia; whereas if barium replaces strontium in equivalent proportions, then since each element occupies practically the same volume, there is no *a priori* reason for any sensible change in crystalline form, which is in accordance with observation.

The conception of specific volume is still further expanded in a lecture given by Kopp to the Chemical Section of the "Naturforscher Versammlung"—the equivalent of our British Association for the Advancement of Science—at Erlangen, in 1840 (*Annalen*, xxxvi. 1 (1840)). He first attempts to prove that the specific weight of isomorphous substances is proportional to their atomic weight; or that ismorphous

bodies possess the same atomic value. Strictly speaking, this law can hold only for those substances which are perfectly isomorphous. The bodies termed isomorphous are, in general, only approximately so, for we find the angles of their crystals deviating several degrees from one another; and the relations between the axes of bodies thus denominated isomorphous are not perfectly equal. The more nearly the crystalline forms of isomorphous substances are identical, the more nearly will their atomic volumes be the same. This is made evident by a comparison of the axial ratios of witherite, strontianite, arragonite, and cerussite; and of the carbonates of zinc and magnesium, mesitin-spar, the carbonates of iron and manganese, and dolomite and calcspar. It is seen that there is a direct comparison in the case of the latter compounds between the length of the principal axis a and the atomic volume V, such that $a^{4\cdot739} = 0\cdot0127671\text{V}$, from which it is possible, of course, to deduce the specific gravity of the substance from its crystalline form. It also follows that an increase of atomic volume is occasioned by an increase in the length of the axis a. If we heat one of these crystals the density decreases, the axis a must therefore increase in length, whilst the angle R becomes less obtuse. This fact, indeed, was discovered by Mitscherlich, who found that the specific gravity of calcspar decreased in the ratio of 1 to $\frac{1}{1\cdot001961}$ on being heated through 100°. The specific gravity of calcspar is 2·7220, when $a = 0\cdot8544$ and $\text{R} = 105°\ 5'$. By heating it through 100°, the sp. gr. becomes 2·7167, or its atomic volume changes from 36·73 to 36·80. If we determine the length of the axis a by means

of the above formula, we find it 0·85672, corresponding to an angle R of 104° 57′, or a difference of 8′, which closely agrees with that actually observed by Mitscherlich (*Philosophical Magazine* [3], xviii. 255).

Kopp's memoir, which gave to the specific gravity of a solid and liquid substance a new and important significance, attracted much attention and occasioned no little controversy. Schröder (Poggendorff's *Annalen*, 553 (1840)), starting from the observation of Ammermüller, that equal volumes of the two oxides of copper contain the same amounts of copper and multiple amounts of oxygen, assumed that the volume of the copper, as of the oxygen, is equal in the two substances, but that the amount of the oxygen in the cuprous oxide stands to that in the cupric oxide as 1 to 2. Hence Schröder drew the general conclusion that the same element can have different specific volumes in different compounds, but that the several values for the specific volumes stand in simple relations to each other. He saw in this hypothesis not only an explanation of the condensation which accompanies chemical union, but also a rational basis for the belief that the volume of a compound is equal to the sum of the volumes of its components.

Schröder's ideas, to begin with, had something in common with those of Kopp, but they underwent frequent modifications during the next thirty years. The characteristic manner in which the two philosophers presented their views was thus sketched by Berzelius: —"Schröder is invariably of opinion that he has discovered what is right: to his thinking, the proofs he cites are evident; he is always convinced and insists that his reader is equally so. . . . Kopp, on the

other hand, draws attention not only to that which supports a statement, but also to that which is adverse to it. . . . He searches for truth, but he indicates, without reserve, what he provisionally regards as only probable."—(Quoted by Ostwald, *Stöchiometrie*, p. 620.)

Kopp's conceptions on the subject at this time took more formal shape in the small book he published in 1841, at Frankfort, *On the Specific Gravity of Chemical Compounds.* A summary of this work appeared in the *Philosophical Magazine* for 1842, from which the following abstract is made:—Kopp points out that Schröder's contentions are weakened by the great number of arbitrary assumptions on which they are based. Schröder, in fact, would seem to forget that the probability of a theory becomes less in proportion as the number of assumptions on which it is founded becomes greater. Kopp believed that the state of knowledge at that time did not enable a perfect and consistent theory to be framed; all he asserted was, that one may be proposed of which a measure of its probability may be inferred from the fact that it explains the greatest number of experimental facts by the fewest possible assumptions. The atomic volume of a compound is seldom equal to the sum of the atomic volumes of its elements—that is, the "primitive atomic volumes" calculated from the specific gravity of the elements in the isolated state. If the atomic volume of a compound is greater than the primitive atomic volumes of either of its components, it is impossible, *a priori*, to say whether one or both of the elements are contained in it with an atomic volume different from the primitive value; but it is certain that one element in a compound does not

possess its primitive atomic volume if the atomic volume of the compound is smaller than the primitive atomic volume of that element. It is not as yet possible to state for every compound which element enters into it with its primitive atomic volume, or whether both acquire different ones. It is only possible to infer this with any degree of probability in the case of analogous compounds which have *one* element in common.

Schroder found that if in a series of analogous bodies AO, BO, CO, of which the specific volumes are known, we subtract from these values the primitive atomic volumes of A, B, and C respectively, we obtain a constant remainder. This he found to be the case with the oxides of lead, cadmium, and zinc, and hence he inferred that the metal in these oxides retains its primitive atomic volume. Kopp assumes that this is equally true of the salts of the heavy metals, but with the salts of the metals of the alkalis and alkaline earths this is impossible, as the specific volumes of the salts are, as a rule, smaller than the primitive atomic volumes of the component metals. He has consequently to assume for these metals a specific atomic volume, which, however, remains the same in all the salts. He determines these values as follows :—Suppose M + R to be a compound of a heavy metal, m + R the analogous compound of a light one; suppose A to be the known specific volume of M + R, and a that of m + R, B the primitive atomic volume of M, and b that of m.

Then,

$$M + R = A,$$

and

$$M = B.$$

Therefore the atomic volume with which R is contained in the compound is A − B, say x.

It is assumed that R retains its value in $m + R$,

and since

$$m + R = a,$$

and

$$R = x;$$

therefore atomic volume of m (or b) $= a - x$. The specific volume of CO_2 in the carbonates is assumed to be 24·2, whence we have—

	Mol. vol. calc.	Sp. gr. calc.	Sp. gr. obs.
$PbCO_3$	42·4	6·30	6·43 to 6·47
$CdCO_3$	38·1	4·52	4·42 „ 4·49
$FeCO_3$	31·4	3·70	3·83 „ 3·87
$MnCO_3$	31·9	3·61	3·55 „ 3·59
Ag_2CO_3	44·8	6·15	6·08
$ZnCO_3$	33·6	3·72	4·34 to 4·45

With the exception of $ZnCO_3$, the observed and calculated specific gravities are fairly concordant.

By means of the values for the alkalis and alkaline earths, obtained as described, Kopp deduces the following specific volumes and specific gravities :—

	Mol. vol. calc.	Sp. gr. calc.	Sp. gr. obs.
$BaCO_3$	47·1	4·19	4·24 to 4·30
$CaCO_3$	33·8	2·96	2·70 „ ·300
K_2CO_3	61·6	2·25	2·26
$MgCO_3$	30·6	2·75	2·61
Na_2CO_3	45·0	2·36	2·47
$SrCO_3$	41·5	3·56	3·60 to 3·62

The specific volume of NO_3 is, in like manner, found to be 28·6.

	Mol. vol. calc.	Sp. gr. calc.	Sp. gr. obs.
$Pb(NO_3)_2$	75·5	4·40	4·40
$AgNO_3$	78·1	4·36	4·36

	Mol. vol. calc.	Sp. gr. calc.	Sp. gr. obs.
NH_4NO_3	92·1	1·74	1·74
$Ba(NO_3)_2$	80·1	3·20	3·19
KNO_3	94·7	2·14	2·10
$NaNO_3$	78·1	2·19	2·19
$Sr(NO_3)_2$	77·6	2·84	2·89

In certain of the sulphates, the value of the group SO_4 appears to be 37·8.

	Sp. gr. calc.	Sp. gr. obs.
$CuSO_4$	3·56	3·57
Ag_2SO_4	5·34	5·34
$ZnSO_4$	3·42	3·40
$CaSO_4$	2·90	2·93
$MgSO_4$	2·75	2·61
Na_2SO_4	2·44	2·46

In other sulphates, however, the group SO_4 appears to have the value 29·8.

	Sp. gr. calc.	Sp. gr. obs.
$PbSO_4$	6·32	6·30
$BaSO_4$	4·43	4·45
K_2SO_4	2·60	2·62
$SrSO_4$	3·90	3·95

In the chromates, $CrO_4 = 36·5$; and in the tungstates, $WO_4 = 39·0$.

	Sp. gr. calc.	Sp. gr. obs.
$PbCrO_4$	5·98	5·95
K_2CrO_4	2·69	2·64
$PbWO_4$	8·04	8·00
$FeWO_4$	6·67	7·10
$CaWO_4$	6·05	6·04

Cl, like SO_4, appears to have two distinct values, viz. 15·7 and 19·8.

	Sp. gr. calc.	Sp. gr. obs.
$PbCl_2$	5·60	5·68
$AgCl$	5·50	5·50
$BaCl_2$	3·83	3·86

	Sp. gr. calc.	Sp. gr. obs.
$NaCl$	2·25	2·26
NH_4Cl	1·44	1·45
$CaCl_2$	2·29	2·27
KCl	1·94	1·94
$CuCl_2$	3·70	3·68
Hg_2Cl_2	6·90	6·99
$HgCl_2$	5·05	5·14
$SrCl_2$	2·80	2·80

The density of many oxides may be calculated on the assumption that the specific volume of O = 5·1.

	Sp. gr. calc.	Sp. gr. obs.
PbO	9·55	9·50
CdO	7·05	6·95
CuO	6·53	6·43
MnO	5·87	4·73
HgO	10·9	11·00
ZnO	5·48	5·43
SnO	6·28	6·67
MoO_2	6·01	5·67
TiO_2	4·16	4·18
PbO_2	8·40	8·90
Sb_2O_3	5·69	5·78
Pb_2O_3	8·91	8·94
Fe_2O_3	5·31	5·25
Co_2O_3	5·64	5·69
$\left.\begin{matrix}Fe\\Ti\end{matrix}\right\}O_3$	4·78	4·78
Bi_2O_3	8·09	8·17

The agreement, with the exception of the cases of MnO, SnO, MoO_2, and PbO_2, is fairly good.

In other oxides, O = 2·55, and in a third group 10·2, all the values, it will be observed, being multiples of a common number.

	Sp. gr. calc.	Sp. gr. obs.
SbO_2	6·53	6·53
SnO_2	7·03	6·96
Cr_2O_3	5·39	5·21

	Sp. gr. calc.	Sp. gr. obs.
Cu_2O	5·87	5·75
Hg_2O	10·5	10·69
Ag_2O	7·48	7·25
MoO_3	3·44	3·46
WO_3	5·68	5·27

The specific gravity of a large number of the sulphides may, in like manner, be calculated, with a similar degree of approximation to the observed values, by assuming that sulphur, like its analogue, oxygen, has three values, viz. 8·5, 15·0, and 17·6.

Kopp also showed that the densities of the hydrated oxides, and of a number of hydrated salts, may be calculated with considerable accuracy by assuming certain definite values for water in a state of combination. It ought to be stated, however, in this connection, that subsequent researches have indicated that Kopp's conclusions respecting the specific volume of water of crystallisation must be slightly modified. Schiff, many years ago, showed that the members of certain classes of hydrated salts have practically the same specific volume. Thus, all the alums have a specific volume of about 277; double sulphates of the form $M_2M''(SO_4)_2.6H_2O$, have a common volume of 207; and all the vitriols—that is, salts of the form $M''SO_4.7H_2O$—whether isomorphous or not, have the specific volume 146.

Lord Playfair sought, many years ago, to determine the precise relation of the specific volume of a salt to its degree of hydration. This work, the natural sequence of the memorable investigations which we owe to Playfair and Joule, was first made known through the medium of a paper communicated to this Society by your lecturer and Mr. John I. Watts ("On the Specific Volume of Water of Crystallisation," *Trans.*

xxxvii. 102 (1880)). On learning that we were engaged on this question, Lord Playfair placed the very ample notes of the investigation, which he was not able to complete, at our disposal, and although, at his suggestion, we went over the whole of the ground again, much of the experimental matter of our communication was based upon his previous observations. The main result of the inquiry was to show that the volume occupied by the several molecules of water varies with the degree of hydration of the salt. In the case of the so-called magnesian sulphates, the first molecule of water, the constitutional water, or "water of halhydration," of Graham, occupies considerably less bulk than the remaining molecules; its mean relative value is 10·7. Each additional molecule appears to occupy a gradually increasing volume. The difference between the monohydrate and dihydrate is 13·3; between the dihydrate and trihydrate it is 14·5; between the trihydrate and tetrahydrate it is 15·4; and between the hexhydrate and heptahydrate it is 16·2. These observations are so far in harmony with Kopp's general conclusions that in the substances containing only a small number of water molecules (1 to 3) the specific volume of the water is 12·4; in others containing a larger number (2 to 7) it is 13·4; whereas in a third class, containing the largest number (from 3 to 10), its mean value is 15·3. It need hardly be pointed out that the main conclusion agrees with general experience. Chemical combination results, as a rule, in contraction, and the stability of the compound is connected with the degree of this contraction. Now, it is well known that the different molecules of water in a hydrated salt are held with varying degrees of tenacity, as shown by the different amounts of heat

required to expel them, and the different amounts, too, which are evolved in the rehydration of the salt.

Kopp finally seeks to disprove the assumption of Schröder that the specific volume with which an element is contained in a compound bears a simple relation ($\frac{1}{2}$, $\frac{1}{3}$–$\frac{1}{9}$; $\frac{2}{1}$, $\frac{2}{3}$–$\frac{2}{9}$; $\frac{3}{1}$, $\frac{3}{2}$–$\frac{3}{9}$) to its primitive specific volume, an assumption which plays a not inconsiderable part in Schröder's subsequent papers, and in those of Hermann (*J. pr. Chem.* xiii. 28 (1876)) and E. Wilson (*Proc. Roy. Soc.* xxxii. 457).

Kopp had hitherto mainly concerned himself with the study of inorganic compounds, and for the most part with solid substances. He next turned to the consideration of organic bodies, and in his papers of 1842, on the "Vorausbestimmung einiger physikalischen Eigenschaften bei mehreren Reihen organischer Verbindungen" (*Annalen*, xli. 79; *ibid.* xli. 169), we have the first of a series of memoirs, on which his fame as an observer will ultimately rest. For his purpose, the study of organic compounds presented many features of importance. In the first place, the variety of elements which enter into the composition of organic compounds is much smaller than in inorganic substances; and in the second, there are more liquid substances and a wider range in the mode of combination among the compounds of carbon than of any other element. These liquids, too, may for the most part be easily obtained of a sufficient degree of purity; they are readily characterised, and the determination of their specific gravity can be made with a higher degree of precision than in the case of solids. On the other hand, liquids are more expansible by heat than solids, and the question of the conditions under which specific volumes are comparable becomes of much greater importance in the one case

than in the other. The main conclusion of this paper may be stated as follows:—If we imagine a number of analogous organic compounds arranged under the following scheme:—

$A+\alpha$	$B+\alpha$	$C+\alpha$	$D+\alpha$, ...
$A+\beta$	$B+\beta$	$C+\beta$	$D+\beta$, ...
$A+\gamma$	$B+\gamma$	$C+\gamma$	$D+\gamma$, ...
$A+\delta$	$B+\delta$	$C+\delta$	$D+\delta$, ...
. . . .	. . . .	. . . .	. . . .
. . . .	. . . .	. . . .	. . . .

where A, B, C, D, α, β, γ, δ, are certain bodies or combinations of elements, then *it is only necessary to become acquainted with one horizontal and one vertical series to know the most important physical properties of all the combinations contained in such a table. If the properties of the compounds contained in one horizontal or in one vertical series are known, the mere knowledge of one of these compounds is sufficient in order to ascertain the properties of all the compounds arranged in any other horizontal or vertical series.*

If the specific volumes and the boiling points of the compound $A+\alpha$, $A+\beta$, $A+\gamma$, etc., are known, and we also know the specific gravity and boiling point of $B+\alpha$, we can at once ascertain the specific gravities and boiling points of the bodies $B+\beta$, $B+\gamma$, $B+\delta$. For between the specific volumes and boiling points of

$A+\alpha$ and $B+\alpha$	$B+\alpha$ and $D+\alpha$	$A+\alpha$ and $A+\delta$,
$A+\beta$ and $B+\beta$	$B+\beta$ and $D+\beta$	$B+\alpha$ and $B+\delta$,
$A+\gamma$ and $B+\gamma$	$B+\gamma$ and $D+\gamma$	$C+\delta$ and $C+\delta$,
etc.	etc.	etc.

the differences are the same.

A similar regularity occurs in the physical properties in cases of substitution, as, for example, that of chlorine

for hydrogen. Let A represent a combination of oxygen and carbon which remains unaltered, α hydrogen, β chlorine, m, n, x, y numbers; we have then—

$$\begin{aligned} &A + m\alpha \quad\quad + n\beta, \\ &A + (m-x)\alpha + (n+x)\beta, \\ &A + (m-y)\alpha + (m+y)\beta. \end{aligned}$$

The sum of the atoms α and β remains the same in each compound. If in any compound x atoms of hydrogen are replaced by x atoms of chlorine, the specific volume of the new compound is increased by $x.12{\cdot}5$ (approximately).

The numerical basis upon which these generalisations rest was undoubtedly affected by the fact that Kopp at this period was unable to determine the precise conditions under which the comparisons of specific volume are valid; indeed, he is disposed to recognise in this circumstance the chief ground for the differences between the observed and theoretical values. But, in reality, the nature of the experimental material itself was hardly sufficient to afford absolute proof that Kopp's laws were capable of the mathematical precision with which he stated them. This fact was, indeed, recognised by Liebig in a note which he appended to Kopp's paper in the *Annalen*, and in which he called upon his fellow-workers to give increased attention to the ascertainment of those physical constants which serve to distinguish a chemical compound. Kopp's generalisations were undoubtedly first approximations to the expression of physical truths, but the limits of their application could only be determined by fuller and more accurate data, and by a clearer conception of the conditions under which the comparisons should be made.

In Kopp's next memoir, "Ueber den Zusammenhang zwischen der chemischen Constitution und einigen

physikalischen Eigenschaften bei flüssigen Verbindungen" (*Annalen*, l. 71 (1844)), this last problem is more particularly attacked. The specific volumes of liquids are obviously dependent on heat; hence they may be said to be comparable at temperatures at which heat exercises the same effect upon the liquids, say at their boiling points, or, in general, at temperatures at which their vapour pressures are equal. Fifty years ago, however, the vapour pressures of but three liquids were known, and these only with approximate accuracy, viz. water, alcohol, and ether; and hence, since Kopp assumed that the specific volume of alcohol was the sum of the specific volumes of water and ether, there were only two equations possible to determine the three unknown values of C, H, and O at all temperatures below the boiling points. On the assumption—which observation showed to be probable—that one atom of oxygen can replace two atoms of hydrogen in a compound without sensible alteration in specific volume, that the comparison may be made at equal distances of temperature from the boiling point instead of at the temperature of equal vapour pressures, and that the elements in combination have the same inherent expansibility, or, in other words, that the relation between the specific volumes of (C), (O), and (H) in a compound is the same at all distances from the boiling point, Kopp was able to deduce values for C, H, and O which enabled the specific gravity of a liquid consisting of these elements to be calculated at any temperature below its boiling point with a certain approximation to accuracy. He found that the specific volume of a liquid compound, consisting of (*a*) atoms of carbon; (*b*) atoms of hydrogen; (*c*) atoms of oxygen; (*d*) atoms of chlorine; (*e*) atoms of nitrogen; and (*f*) atoms of

sulphur, is, for a temperature distant D° from its boiling point, approximately expressed by

$$(1{\cdot}28a + 0{\cdot}48b + 0{\cdot}96c + 2{\cdot}34d + 1{\cdot}44e + 2{\cdot}4f) \times (9{\cdot}75 - 0{\cdot}01\mathrm{D}).^{1}$$

Kopp warns his reader that this formula has no other value than as expressing the specific volume, and, indirectly, the specific gravity, of a liquid consisting of the above-named elements, with, on the whole, a fair degree of accuracy. He is careful to point out that it rests on assumptions for which definite experimental evidence is, in great measure, wanting. If both the form of the formula and the values of its constants were accurate, it would be possible to infer the boiling point of a liquid from a knowledge of its composition and its specific gravity. It would also follow, from the nature of the formula, that the contraction which a liquid experiences on cooling through X° from its boiling point was invariably proportional to X; whereas observation shows that the contraction for a given temperature interval becomes smaller and smaller as the distance from the boiling point increases. Moreover, it assumes that two liquids, having the same volume at the boiling point, must necessarily possess the same volume at D° from the boiling point, which is also contrary to experience.

Kopp ventured to conclude, then, that in all probability—

1. Equal differences in composition corresponded with equal differences in specific volume.
2. Equivalent amounts of oxygen and hydrogen in liquid compounds occupy nearly the same volume.
3. The specific volume of a compound is equal to the sum of the specific volumes of its com-

[1] The values were originally calculated on the basis of Berzelius's atomic weights; they have been transformed to the corresponding values when H = 1.

ponents. The same element almost invariably preserves the same specific volume. Isomeric compounds have the same specific volume; polymeric compounds have specific volumes which stand to one another in the same relation as the molecular weights of the compounds. Variations in the chemical constitution of isomeric compounds are without effect on their specific volume.

4. Comparisons of specific volumes of liquids are only valid at temperatures at which the vapour pressures of the liquids are equal.

These conclusions, as Kopp clearly recognised, rested on no very firm experimental basis; the necessary data, indeed, were not to hand to test them rigorously. Accordingly, he resolved to make accurate determinations of all the physical constants needed to ascertain, with the highest possible precision, the specific volumes of a large number of liquids.

In 1855 Kopp published the first of the series of memoirs in which he gave the results of his ten years' experimental labours. These memoirs are, without doubt, the most remarkable and most characteristic of his scientific papers. They exhibit in a striking degree all the attributes of a successful investigator—ingenuity, manipulative skill, a rigid sense of accuracy, illimitable patience, insight, and inductive power.

His main conclusions may thus be summarised :—

1. The selection of the temperature of equal vapour tension as a basis of comparison seemed to be warranted by the fact that regularities are thereby made evident which otherwise are not apparent.

2. Differences of specific volume are proportional to differences in chemical composition.
3. Isomeric liquids of the same chemical type have equal specific volumes.
4. The substitution of hydrogen for an equivalent amount of oxygen only slightly affects the specific volume.
5. One atom of carbon can replace two atoms of hydrogen without alteration in specific volume.

It was further shown that the specific volume of a liquid was determined, not only by its composition, but also by its constitution. Kopp found, for example, that the relative position of the oxygen atom in a molecule affected its specific volume; carbonyl-oxygen and hydroxyl-oxygen have two very different values. Sulphur, in like manner, would appear to have two specific volumes, depending upon its position or mode of combination in a molecule.

Definite values for the specific volumes of carbon, hydrogen, and oxygen were obtained from the following considerations:—An increment of CH_2 corresponds to an increase of specific volume of 22. Since C and H_2 occupy the same volume, we have $C = 11$ and $H = 5{\cdot}5$. The replacement of H_2 by carbonyl O is attended by a slight increase in the specific volume. Kopp found that the most probable value for this form of oxygen was 12·2. For hydroxyl-oxygen it is 7·8, obtained by subtracting the value for H_2 (11) from the specific volume of water, 18·8. Hence the specific volume of a compound $C_aH_bO_cO'_d$, where O is carbonyl-oxygen and O′ hydroxyl-oxygen, may be expressed by the formula

$$V = 11{\cdot}0a + 5{\cdot}5b + 12{\cdot}2c + 8{\cdot}7d,$$

which gives results in no case varying more than 4 per

cent from the observed value, and, in the great majority of cases, much less.

Determinate values for the halogens, and, with less precision, for phosphorus, arsenic and antimony, silicon, titanium, and tin, were also obtained. As regards nitrogen it was found that in the amines $N = 2{\cdot}3$. The group $CN = 28$; that of $NO_2 = 33$. If, then, carbon and oxygen preserved their ordinary values in these radicles, N must possess at least three different values depending on the mode of combination. There is, however, no evidence to disprove the supposition that the values for the carbon and oxygen atoms are not equally affected in these groups. This, indeed, suggests the possibility that compound radicles like CO, HO, NO_2, CN, etc., may possess definite specific volumes which are not necessarily the sum of the specific volumes of the component atoms as ordinarily ascertained.

It remains to indicate how far these conclusions have stood the test of criticism and subsequent research, especially when viewed from the standpoint of modern dynamics and in the light of our present conceptions concerning constitution. In 1865 H. L. Buff (*Annalen*, iv. (Suppl.), 129) sought to show that the specific volume of carbon, like that of oxygen and sulphur, is affected by its mode of combination; or, in other words, that carbon in unsaturated compounds has a greater specific volume than in saturated bodies; from which he surmised that the specific volume of an element is in general determined by its particular atomic value. With this exception, the subject received practically no further experimental treatment until about 1880, when a number of papers followed each other in rapid succession. Only the briefest summary is here possible. It was found that isomeric liquids have not invariably the same specific

volume. Between ethylene and ethidene chlorides, for example, there is a greater difference than 4 per cent; indeed, ethylene compounds in general appear to have smaller volumes than those calculated by Kopp's values. Städel further showed that in the series of the chlorinated and brominated ethanes and ethylenes, the isomer of higher boiling point, *i.e.* the ethylene derivative, has invariably the lower specific volume. Since these compounds are all saturated and the only variable constituent is a monovalent element (Cl or Br), it would appear probable that the specific volume of the halogen is also variable. Isomeric hydrocarbons manifest similar differences, whence it is obvious that either one or both of the elements must have a slightly variable volume, depending on grouping or mode of combination ; it may be that the iso-group, like the groups carbonyl, hydroxyl, nitryl, etc., has a special volume, which is not necessarily the sum of the volumes of the component atoms as deduced from Kopp's values. Kopp himself found that the volumes of isomers were, in a number of cases, only approximately equal, and in others quite unequal. The term "chemical type," used in the sense in which Gerhardt employs it, is not sufficiently distinctive to denote the differences, say, between the normal and iso-compounds, or between aniline and picoline, and it is questionable whether Kopp would have considered such cases as coming within his rule.

The observed specific volumes of the aromatic compounds, also, are frequently lower than the calculated values. Indeed, our views as to the constitution of the aromatic compounds would lead us to expect that the specific volume of benzene (and the derivatives in which the benzene group functions) would probably be different from that deduced from

observations made, for the most part, on compounds of totally different constitution. Kopp (*Annalen* (Suppl.), v. 303 (1867)) showed from Louguinine's observations that whilst benzene has an abnormally low specific volume, its homologues show the constant increase of 22 for an increment of CH_2, which is what might be anticipated, since these bodies are produced by the addition (substitution) of methyl, ethyl, etc., to the benzene group. Jungfleisch's observations on the specific volumes of the chlorine substitution products of benzene also seem to show that the positions of the chlorine atoms affect, in a very marked manner, the specific volume of the product (*Compt. rend.* lxiv. 911). Further observation has shown that Kopp's conclusion, that liquid elements and radicles have the same volume in combination as in the free state, is well founded. Thus the observed volume of $NO_2 = 32{\cdot}0$ —calculated, 31·5; observed volume of $Br = 53{\cdot}6$—calculated, 53·4; observed volume of $CN = 28{\cdot}9$—calculated, 28·9. The observed specific volume of Cl from Knietsch's careful determinations of the specific gravity of liquid chlorine is 22·8; the mean calculated value is 22·7. Kopp surmised that members of the same chemical family would be found to have the same specific volume; observation shows, however, that the specific volumes gradually increase with the increase of atomic mass.[1]

Thanks to Roberto Schiff, Lothar Meyer, and his pupils, and especially to Lossen and his pupils, a very large amount of experimental material has been accumulated during the last ten years, from which important

[1] Thorpe: "On the Relation between the Molecular Weights of Substances and their Specific Gravities when in the Liquid State," *Trans.* 1880, xxxvii. 141; *ibid.* 327.

deductions may be drawn as to the general validity and limitations of Kopp's conclusions. Schiff (*Annalen*, ccxx. 71 (1883)), by means of an improved form of an apparatus first suggested by Ramsay (*Trans.* 1879, xxxv. 463), concluded that whilst it is generally true that isomeric compounds have slightly different specific volumes, it is almost invariably the case that the substance possessing the higher boiling point has also the higher specific volume (compare Städel). In the case of the metameric esters of the fatty acids, it is found that, as a rule, the specific volumes increase with the diminution of the number of carbon atoms in the acid radicle and with the increase in the alcohol radicle. At the same time it would appear that the differences between the observed and calculated values are mainly due to the alcohol, the acid apparently having but slight influence. This is in conformity with Lossen's observations, that whilst the ethers and acids give experimental values which are almost in exact accordance with Kopp's values, the aldehydes and alcohols show wider variations, methyl alcohol giving too great an observed value, whilst the others give smaller and smaller values as the amount of carbon increases. It is, however, noteworthy that the differences between the aldehydes and derived alcohols remain almost constant, which is not the case with the aldehydes and acids, where it appears to increase with the molecular weight. Hence the differences between the homologous aldehydes are very nearly equal to those between the corresponding homologous alcohols (Lossen). The mode in which carbon is combined in an organic compound has, according to Schiff, a distinct influence on its specific volume; like Buff, he finds that double-linked carbon occupies a smaller volume than when single-linked.

It is, however, very doubtful whether the facts at present known are sufficient to establish this conclusion.

The examination of the very large quantity of experimental material which is now before us forces us to the conclusion that molecular volume is not a purely additive property. There is no longer room for doubt that the molecular volumes of substances are affected by far more conditions than we have hitherto taken cognisance of. The value $CH_2 = 22$ has no other significance than as expressing the average increment in volume in successive members of a homologous series. Indeed, as the physical data increase, it becomes doubtful whether even this mean value is correct. Later observations appear to show that the value augments as the series is ascended. The relation $C = 2H$ no longer applies to carbon compounds in general. What is true of carbon and hydrogen is equally true of oxygen, whether as carbonyl- or as hydroxyl-oxygen. No definite or uniform values can be assigned to oxygen such that the molecular volume of a liquid can be *a priori* determined. The values given by Kopp are simply mean values, but the actual volumes are affected by conditions of which, as yet, we have no very precise knowledge or any certain means of measuring. The values for the other elements are, of course, affected by these considerations. Thus the specific volume of chlorine is obtained on the assumption that the values for carbon and hydrogen are constant. All, then, tends to show that the molecular volume is not the sum of constant atomic volumes.

Lossen has attempted, and with considerable success, to devise formulæ which shall take note, or express the measure, of the influences which affect the uniformity

in the values of specific volumes in organic compounds (*Annalen*, ccliv. 42). These formulæ, as yet, can only be considered as first approximations, but their value will be evident from the fact that they serve to reproduce the observed values with a greater approach to accuracy than has hitherto been possible. Out of the 407 compounds which constituted the experimental material on which these formulæ are based, the observed molecular volumes of 352 differ by less than 2 per cent from the calculated volumes. Comparatively few of these differences may be ascribed to experimental errors. In the main, they are caused by influences of structure and composition which, as yet, we have no certain means of measuring, such as the effect of substituted chlorine, or the special effect of the iso-group, or of the ortho-, meta-, or para-position, etc.

No summary of the present state of our knowledge respecting this question would be complete without some reference to Schröder's later views. For nearly fifty years Schröder laboured unremittingly to throw light on the connection between volume and molecular weight, and the final form of his theory is too suggestive to be passed over in silence. Schröder regards the atomic volume of an element in combination as variable within limits determined by the nature of the chemical compound. In any one compound, however, all the elementary atoms occupy either equal or multiple volumes. Hence every molecular volume is a multiple of a certain *space-unit* or *stere*, the value of which may vary between 6·7 and 7·4, depending on the number, nature, and mode of union of the atoms. Ostwald, to whom I am indebted for this presentation of Schröder's argument, thus explains how Schröder obtains his determinate

relations. The molecular volumes of formic, acetic, and propionic acids increase about 22 units for each increment of CH_2; in the case of alcohols the increase is about 20 units. The volume of formic acid is 41·8—that is, $2 \times 20{\cdot}9$; that of methyl alcohol is 42·7 or $2 \times 21{\cdot}4$. Hence it follows that in formic acid, CH_2O_2, the O_2 occupies the same volume as CH_2; and in methyl alcohol, CH_2O, the H_2O has the same volume as CH_2. In like manner, ethyl alcohol, C_2H_6O, has the volume 62·2—that is, $3 \times 20{\cdot}7$. Since $2 \times CH_2 = 2 \times 20{\cdot}7$, then $H_2O = 20{\cdot}7$. Acetic acid has the volume 63·5—that is, $3 \times 21{\cdot}2$; $2CH_2$ is $2 \times 21{\cdot}2$ and $O_2 = 21{\cdot}2$. The volume of acetaldehyde is 56·9—that is, about 6·6 smaller than that of acetic acid; this would indicate that the substitution of OH by H lowers the volume by 6·6. Since $H_2O = 21{\cdot}4$, it follows that the hydrogen and oxygen in hydroxyl each occupy one space-unit or stere. As $CH_2 = 21$, we find that carbon also occupies one stere or space-unit. Now from the volume of formic acid, which contains 6 steres, it is found on subtracting 3 steres for CH_2 and 1 stere for hydroxyl-oxygen, that the carbonyl-oxygen must occupy 2 steres.

We obtain then the following rule:—*The number of space-units or steres of the saturated compounds of carbon, hydrogen, and oxygen corresponds to the number of the atoms, increased by as many units as there are atoms of carbonyl-oxygen present.*

If we calculate, by means of this rule, the steres of the saturated compounds, we find that they vary within narrow limits, and for the most part increase with increasing molecular weight. In the greater number of cases, the values range between 6·9 and 7·2. Ostwald has calculated the value of the stere

for a large number of aliphatic compounds with the following results :—

Hydrocarbons, 6·89 ; 6·99 ; 6·82 ; 7·11 ; 7·23.
Alcohols, 7·12 ; 6·91 ; 6·77 ; 6·88 ; 6·78 ; 6·78 ; 6·81 ; 6·74 ; 7·09.
Acids, 6·97 ; 7·06 ; 7·11 ; 7·19 ; 7·24 ; 7·24 ; 6·85.
Esters, 7·04 ; 7·08 ; 7·05 ; 7·14 ; 7·26 ; 7·43 ; 7·45 ; 7·47.
Aldehydes, 7·11 ; 7·05 ; 7·18 ; 7·01 ; 7·18 ; 6·93 ; 7·27.

In the series of the hydrocarbons, the acids, and the esters, the steres, in the case of the normal compounds, increase regularly with increasing molecular weight; in that of the alcohols they decrease up to the third member and then increase. The secondary and tertiary compounds have, as a rule, smaller steres than the normal compounds. It is instructive to note the different manner in which Kopp and Schröder arrived at the volume relations of these elements. Kopp concluded from the approximately equal volumes of the alcohols and corresponding acids that H_2 and O are volumetrically equivalent; and from the equivalence of the volumes of benzyl and amyl compounds he inferred that C_2 and H_4 are mutually replaceable without alteration of volume; hence he assumed that the hydrogen atom occupies only half the volume of the oxygen or carbon atom. Schröder establishes the volume equivalence of CH_2, HOH, and O'O, and he inferred from the difference in volume between alcohol and aldehyde that hydroxyl oxygen has the same volume as hydrogen and carbon, whilst carbonyl-oxygen has twice the volume. Whilst then Kopp assumes (approximately) $H_2 = C = O$, Schröder makes $H = C = O$.

In the case of unsaturated and aromatic compounds, Schröder assumes that each double linkage is attended with an increase of volume amounting to one stere; hence the foregoing rule has to be modified in this

sense when applied to compounds of this class. The value of the stere in a number of unsaturated compounds is found to be—

Hydrocarbons, 6·87 ; 7·09 ; 6·93 ; 6·99.
Alcohols, 6·72.
Esters and ethers, 7·14 ; 7·13.

The values vary, practically, within the same limits as in the aliphatic compounds.

As regards aromatic compounds, it would seem to follow, from the identity in the volumes of benzoyl and amyl compounds, as indicated by Kopp, as well as from the corresponding relation between the isobutyl and phenyl compounds, that C_6H_5 comprises the same number of steres as C_4H_9—that is, 13. Of these, 5 are occupied by hydrogen; so that the carbon group C_6 occupies 8 steres.

The values of the stere in a number of aromatic compounds are as follows:

Hydrocarbons, 6·85 ; 6·94 ; 6·98 ; 7·00 ; 6·95 ; 7·04 ; 7·04 ; 7·04 ; 7·06 ; 6·84.
Other compounds, 6·91 ; 6·87 ; 7·05 ; 6·97 ; 7·16 ; 7·26 ; 7·50 ; 7·28 ; 7·14 ; 6·96 ; 7·07.

The value of the stere here also varies within the usual limits; it is comparatively small for the hydrocarbons ($6·8 - 7·0$), larger in the case of the alcohols, and still larger in that of the esters ($7·2 - 7·5$). These relations, if substantiated, would seem to indicate that the law of volumetric combination among the masses of liquid molecules is essentially not more complex than the law of combination between those of gaseous molecules.

There is one consideration which is vital to the whole question, and to which, therefore, a brief reference

must be made. It relates to the choice of conditions under which the values we term molecular or specific volumes are really comparable. Although Horstmann and Lossen have advanced reasons against the practice, contending that at any other temperature, say 0°, relations similar to those now established are made manifest, it has been the custom, in accordance with Kopp's direction, to compare the specific volumes of liquids at the temperatures of their respective boiling points under a standard atmosphere. Whether, however, the temperature of the boiling point, under these circumstances, is a truly comparable condition is open to question. It has been urged by Horstmann that, since what we call atomic volume is the space not merely *filled* by an atom, but also that in which "it moves, and lives, and has its being," it is not *a priori* probable that a temperature which differs, say, by 300°, as for example in the case of C_4H_{10} (boiling point 1°) and $C_{18}H_{38}$ (boiling point 317°), these volumes will be the same. Moreover, as pointed out by Bartoli, the boiling point cannot, in the nature of things, be a strictly comparable condition, since it is affected by pressure to a different extent in the case of different liquids. Objections of even greater weight may be urged against the suggestions of Tschermak and Krafft, to take the melting point as a comparable state.

No doubt, theoretically speaking, a valid condition should be when pressure, volume, and temperature are expressed in terms of their critical values. But that certain regularities in the molecular volumes at the boiling points have, in spite of this, been discovered, may be explained, as Guldberg has shown, when we compare the values of T_1, the absolute boiling point, with those of T, the absolute critical temperature; in

those cases in which these two constants are known, the ratio T/T_1 approximates to $\frac{2}{3}$. Hence it follows that qualities like molecular volumes, which alter only slowly with temperature, are comparable at the ordinary boiling points (*Zeits. für Physikal Chem.* v. 374).

It ought perhaps to be stated that subsequent observations show that the so-called "corresponding temperatures" deduced from Van der Waal's generalisations have not that degree of validity as temperatures of comparison which they were originally assumed to possess. Indeed, the present condition of knowledge warrants the statement that Kopp's original method of comparison is as valuable as any yet indicated.

Kopp continued to the last to interest himself in the problem which had been the mainspring of his scientific activity. Shortly before his death he gave to the world, through the *Annalen* (ccl., 1889, 1) a critical account, written with the dignity and calm that befits the well-earned leisure of a veteran controversialist, of the many strivings which had been made to solve it since the tentative efforts he put forth in his thesis of 1838.

We rise from the perusal of this memoir with the conviction that, after all, the work thus summarised takes us but little beyond the threshold of the fundamental truth of which its author was the first to perceive the indication.

As yet we see through a glass darkly, and know only in part; but with the fuller light of a rapidly advancing knowledge, we shall most certainly get an insight into the causes which affect the universality of Kopp's conclusions. The discrepancies, if we could only read them aright, contain within them the clues

to a broader generalisation which will more clearly connect the chemical nature of molecules with their physical attributes. Our experimental material will soon be sufficient for the basis of this generalisation, even if it is not so already. What is wanted is another Kopp to interpret it correctly.

XIII

VICTOR MEYER

An Address delivered to the Fellows of the Chemical Society, 8th February 1900[1]

By the untimely death of Victor Meyer, on 8th August 1897, under circumstances of peculiar sadness, and at the comparatively early age of forty-nine, our sister Society in Berlin lost her President of the year, and, at the same moment, we were deprived of one of the most brilliant of that band of eminent men whom we distinguish as our Honorary Foreign Members. The Council have deemed it fitting that the attempt should be made to put on record our appreciation of the remarkable services Victor Meyer rendered to the science which he cultivated, during the all too short period of his activity, with such striking assiduity and success. As a friend of nearly thirty years' standing, and as one who worked, literally, side by side with him in the famous laboratory which he lived to direct, and died whilst directing, I have charged myself with the execution of this duty.

Of Meyer's early life—that is, of the period before I first knew him at Heidelberg as a girlish-looking, bright-eyed youth, quick of movement and active in thought, ready and fluent of speech, full of zeal, and

[1] Chemical Society's Memorial Lectures, No. IX.

intensely interested in the higher work of the place—I know little, beyond that he was born in Berlin and was the son of a calico manufacturer.[1] Once, in the course of some discussion on the characteristic differences in the school training of English and German lads, he made reference to his own experiences in the gymnasium, from which I gathered that his inclination towards science was in nowise shaped by what he saw or heard in early youth. Nor, so far as can be determined, was there anything in his home life which inclined him to take to chemistry. In the case of many who have become eminent in physical science—and doubtless also in the case of more who have not—their first love has sprung from the passion of experimenting But at this time Meyer, apparently, had neither the opportunity nor the desire to make experiments. Indeed, the home atmosphere tended to make him literary or artistic. There can be no doubt that he owed to this environment, and more especially to the example and precept of his mother, herself a woman of considerable intellectual power, certain strongly marked features of character which are not usually associated with men of science. According to his friends Liebermann and Jacobson, his own wish was to become an actor, and that he would have succeeded in such a profession is highly probable. He had, indeed, all the natural gifts of the born actor—dramatic sense, emotional power, a fine voice, an impressive manner, and a handsome presence. When he became a teacher of chemistry these attributes were turned to good account. His early love of declamation, combined with his elocutionary ability, eventually made him one of the most striking and effective lecturers in

[1] The date of his birth was 8th September 1848.

Germany. The traditions of the University lecture-theatre no doubt exercised their restraining influence, but Meyer was too much under the sway of his artistic temperament and too impatient of conventionality to repress altogether his natural bent. Moreover, he was not insensible to the effect he created, or unmindful of the influence he gained, when, to use the common phrase, he "let himself go," and by his trenchant, impressive language, and the brilliancy of his illustration, communicated something of his own enthusiasm to even the most listless of back-bench men. This power of exposition was quickly perceived, and no doubt its early recognition served to bring him the more speedily to the front.

At Heidelberg, as in many other centres of chemical instruction, there was a small Chemical Society, composed of the Extraordinary Professors, the Privatdocenten, and assistants, together with the senior or more active students in the various laboratories who were elected into it by favour of the teachers. In my time it numbered amongst its members Erlenmeyer, Ladenburg, Horstmann, Ludwig, Cohen (the mineralogist), Rose, and Emmerling. Its president was Bunsen, and the occasions on which he took the chair were the red-letter days of the session. We invariably sent him home happy, his pockets filled with all the good champagne corks we could collect. The formal business of the Society—if formal it can be called—was preceded by an *Abendessen*, and if not accompanied, at least succeeded, by a considerable manifestation of *das gemüthliche Element*. No man was more popular at these gatherings than Meyer. His nimble mind and retentive memory, his gift of ready speech, his sense of humour and genial manner combined

to make it pleasant to listen to him no matter whether he was, in accordance with the rules of the Society, called upon to give an account of some work which had just been published, or whether he was discussing and criticising a communication from a fellow-member. From time to time we had reports of the condition of such investigations as were in progress in the Heidelberg laboratories, or of which the results were to appear in the forthcoming issue of the *Annalen*, for, at the period of which I write, the *Berichte* was a thin and puny publication, hardly out of its swaddling clothes, and with little sign of the vitality which has since enabled it to assimilate practically the whole outcome of German chemical activity.

Of his own laboratory work we had nothing from Meyer, for there was little or nothing at the time to be told. He had entered the University in the autumn of 1865, when barely seventeen years of age, and apparently with no very definite conception of a career. Suddenly he elected to study chemistry, and attached himself to Bunsen with the idea of ultimately becoming a teacher. It is not improbable that his choice was in a measure determined by the circumstance that he had attended some of Hofmann's lectures in Berlin in the preceding summer, and had thus been influenced by that great teacher, then in the fulness of his intellectual vigour. Those were the palmy days of Heidelberg—the days of Bunsen, Kopp, Kirchhoff, Helmholtz—and Meyer came under the spell of them all. His progress as a student was exceptionally rapid, and the brilliant manner in which he gained his doctorate—without the adventitious aid of a thesis—strongly impressed the whole philosophical faculty. Bunsen especially was greatly struck with the power

and promise of the young Jew, then one of the youngest students in the University, and soon selected him as one of his assistants. It was in this capacity that I first made the acquaintance of Meyer. Bunsen for some years previously had been engaged in the examination of the mineral waters of South Germany, and Meyer at the time I entered the Heidelberg laboratory was acting as water analyst. However great the disciplinary value of such work might be (and no one who has practised Bunsen's method of water analysis, with its system of check and control, can doubt that it is one of the most rigorous quantitative exercises possible), I fear it was not altogether congenial to the active mind of the young assistant, who was yearning to try his 'prentice hand at original research. Accordingly, towards the end of 1868 he threw up the position, and entered Baeyer's little laboratory at the Gewerbeakademie in Berlin. Although in a sense overshadowed by the more magnificently appointed University laboratories of Hofmann in the Georgenstrasse, and of Kekulé in Bonn, both at that time comparatively new, the modest laboratory of the Gewerbeakademie, with its twenty workers, was already one of the most famous schools of organic chemistry in Europe. Baeyer himself had recently published his brilliant investigation of mellitic acid; and Graebe, at first alone, and subsequently in conjunction with Liebermann and with Caro, was at work on those remarkable series of inquiries which served to establish the true chemical nature and relationships of alizarin, and led eventually, with the independent collaboration of Perkin, to its artificial production on a commercial scale. I well remember the interest and excitement which these

discoveries created in Germany: they unquestionably gave an enormous impetus to the study of organic chemistry and attracted eager aspirants for chemical fame from all parts of the world, quickened, no doubt, by the perception of the rich promise of material benefit thus suddenly opened up. Heidelberg at that period was pre-eminently a school of inorganic chemistry; organic chemistry was represented by Delffs and was mainly studied by prospective apothecaries. Erlenmeyer had been called to the Polytechnic at Munich, and the influence of Kekulé's teaching was as yet hardly felt by any of the Privat-docenten.

Baeyer quickly recognised the power and ability of his pupil, and to Baeyer was undoubtedly due the impulse which started him on his career. As a *Vorgerückter* he attended no more lectures, and thus it happened that he who became one of the greatest organic chemists of his time never followed a course on organic chemistry. Shortly after his entrance into the laboratory he began the series of half-dozen investigations which characterised his activity during his three years' stay in Berlin. His first paper, published in the *Berichte*, was a note on the action of trimethylamine upon monochlorhydrin, which substances form a basic condensation product, the chloride and the gold salt of which he described (*Ber.* 1869, ii. 186). This was quickly followed by a short paper on diethyl thiodicarbonate, $S(CO_2Et)_2$, which he obtained by the action of ethyl chloroformate upon sodium sulphide (*Ber.* 1869, ii. 297).

A far more ambitious production appeared in the following year, dealing with the constitution of the disubstituted benzenes (*Annalen*, 1870, clvi. 265). In this memoir Meyer described a new method of

introducing a carboxyl group into the molecule of aromatic compounds, no matter whether the substance already contained a carboxyl group or not. This was effected by the action of sodium formate upon the potassium salt of the aromatic sulpho-acid.

Of the acids prepared synthetically by this method, isophthalic acid was the most important on theoretical grounds, as its production under these conditions led to a revision of the views then held with respect to the orientation of the radicles in the "ortho" and "meta" (salicylic) series. Isophthalic acid had been shown by Baeyer to be a 1 : 3-derivative. "Ortho"-sulphobenzoic acid, obtained from benzoic acid, was thought to have its radicles in contiguous positions. Meyer's experiments showed that isophthalic acid was the only dicarboxylic acid formed from the sulpho-acid by interaction with a formate, whence he argued that "ortho"-sulphobenzoic acid, and the chloro-, bromo-, nitro-, amido-, and hydroxy-benzoic acids corresponding with it in constitution, must, like the isophthalic acid, be 1 : 3-derivatives. It followed that the "meta" (salicylic) series of compounds are the 1 : 2-compounds.

The main facts of this paper have long since taken their place in the history of our knowledge of aromatic compounds, but the memoir has an especial interest as being Meyer's first excursus into the realm of chemical theory. In a subsequent extension of the work, in conjunction with Ador (*Annalen*, 1871, clix. 1), he showed that in sulphanilic acid the substituent groups SO_3H and NH_2 were in the 1 : 4 positions. The phenol-sulphonic acid from sulphanilic acid he proved to be identical with Kekulé's paraphenolsulphonic acid, and hence to have the position 1 : 4. Potassium monobromobenzoate fused with sodium formate yields

isophthalic acid; hence this bromobenzoic acid belongs to the 1 : 3 series. At the conclusion of their memoir, the authors gave a list of the disubstituted derivatives of benzene then known, arranged in columns according to whether the second substituent element or radicle is attached to the second, third, or fourth carbon atom, as deduced from the experimental evidence put forward. Their views as to the orientation of the substances there named, except in the case of the dihydroxybenzenes, have not been materially modified by subsequent inquiry.

Pending the publication of this work, Meyer essayed to solve that *arcanum* of aromatic chemistry, the constitution of camphor (*Ber.* 1870, iii. 116). He sought to show in the first place that camphoric acid is a dicarboxylic acid, $C_8H_{14}(CO_2H)_2$. Since camphor yields, by the action of dehydrants, an homologue of benzene, namely, cymene, he reasoned that it must contain the benzene nucleus, and hence the remaining four carbon atoms must exist in the side-chains. But by the oxidation of camphor to camphoric acid, the side-chains apparently are not attacked; the action would seem to be on two of the carbon atoms in the benzene ring. The carboxyl groups cannot be attached to one and the same carbon atom, otherwise camphor would not yield an homologue of benzene by abstraction of water. It would follow therefore that, in all probability, camphor ought to have the constitution expressed by one or other of the following formulæ :—

(1) $COH \cdot C_8H_{14} : CH.$

(2) $C(OH) : C_8H_{14} : CH.$

(3) $CH : C_8H_{14} : CH.$
$\quad\quad\quad

Tollens. The second formula, which contains the hydroxyl group, was rendered improbable by Berthelot, and the improbability was further strengthened by Meyer, who found that acetic chloride was without action on camphor. The third formula then would seem of the three to be the most probable. The constitution of the group C_8H_{14} can only be inferred from that of the camphor cymene. This, as shown by Fittig and his pupils, is probably for the most part methylpropylbenzene.[1] Hence camphor would appear to have the constitution

$$CH{:}C(CH_3){\cdot}CH_2{\cdot}CH_2{\cdot}C(C_3H_7){:}CH,$$
$$\text{(O bridge)}$$

and camphoric acid

$$CO_2H{\cdot}C(CH_3){\cdot}CH_2{\cdot}CH_2{\cdot}C(C_3H_7){\cdot}CO_2H.$$

Borneol would be $CH(OH):C_8H_{14}:CH_2$. These views have now only an historical interest. All that is certain in them is that Meyer's conception of camphoric acid as a dicarboxylic acid is correct. The work of Claisen and Manasse has rendered it practically certain, as long surmised, that camphor has the grouping $C_8H_{14}\begin{matrix}CH_2\\|\\CO\end{matrix}$. It has, however, required the labour of a generation of workers, and the accumulation of a literature which, as regards its bulk, is without a parallel in any other department of chemistry, to unravel the true relations of camphoric acid to camphor, and hence to get an insight into the constitution of a substance which has been known in Europe and prized as a medicine since the twelfth century.

Meyer acquired some experience in tuition, even in

[1] Widman (*Ber.* 1891, xxiv. 450) subsequently proved that cymene is methylisopropylbenzene, but this does not affect Victor Meyer's argument.

the early Heidelberg days, as a "coach," and in Berlin he added to his means, which were slender enough, by similar work. His success as a teacher induced Baeyer to recommend him to Fehling, who sought assistance, especially in modern organic chemistry and in laboratory teaching, in connection with his duties at the Stuttgart Polytechnic. His departure from Berlin was a great loss to the little circle in the Gewerbeakademie, where his high spirits and geniality made him universally popular. He was an omnivorous reader and the power of his memory was astonishing, so much so that on his leaving, Baeyer exclaimed "Jetzt werden wir ja wieder die Literatur nachschlagen müssen."

He remained in the Würtemberg capital barely a year. How he came to leave it has already been told by Professor Liebermann. Johannes Wislicenus had just been called from the Zürich Polytechnic to Würzburg, and the President of the school, Kappeler, was engaged in searching for a successor. He came to Stuttgart, and, unknown to Meyer, attended one of his lectures. Although Kappeler, as he himself relates, knew little of chemistry, he was so struck with Meyer's power of lucid and stimulating exposition that the decision to invite him to Zürich was immediately made. At the close of the lecture he communicated to the unsuspecting young teacher his idea, expressing, however, his fear that in comparison with his future students he might prove to be still too young. As Meyer laughingly promised to do his best, day by day, to repair this fault, the invitation was given, and thus, when barely twenty-four years of age, he found himself Ordinarius and Director of the chemical laboratory of the Zürich Polytechnic.

The Zürich Polytechnic has enjoyed a succession of

distinguished teachers, and Meyer worthily sustained and, indeed, greatly extended the fame of its chemical chair. He was now in possession of a well-equipped laboratory, and surrounded by eager, active students, stimulated and encouraged by the enthusiasm of a teacher as active and eager, and, it may be added, as high-spirited as themselves. The thirteen years of Meyer's stay at Zürich constitute the most fruitful and the most brilliant period of his career: before its close, he had brought himself within the foremost rank of contemporary investigators. Some idea of his wonderful power of work, and of the stimulus he gave to others, may be gleaned from the fact that during that period the Zürich laboratory, under Meyer's direction, gave close upon 130 papers and memoirs to chemical literature. It is, of course, impossible here to do more than indicate, in the briefest possible outline, the outcome and significance of the more important of them.

During his short stay at Stuttgart he sent some half-dozen papers to the *Berichte*, some of them in continuation of work which had occupied him in Berlin. The chief of these were put together in a memoir, in conjunction with Stüber, on the aromatic amines, which appeared in 1873 (*Annalen*, 1873, clxv. 161). The main object of the work was to gain further evidence in favour of Meyer's view, that in the case of the chloro-, bromo-, iodo-, and nitro-derivatives of aromatic amines, obtained by direct substitution, it is always the hydrogen immediately contiguous to the amido-group which is replaced, or, in other words, that the NH_2 group exercises an attractive influence on the substituent. It was assumed by Kekulé that Riche and Bérard's dibromaniline, obtained by the reduction of dibromonitrobenzene, was identical with the

dibromaniline from acetanilide. Meyer and Stüber proved that such was not the case. It was found that Riche and Bérard's dibromaniline yields, or is derived from, a *liquid* dibromobenzene, which boils at 219·4°, and remains liquid at −28°. Riese had previously obtained a liquid dibromobenzene boiling at 224° and crystallising below −1°. All the three possible dibromobenzenes were thus made known. Meyer's surmise that the new dibromobenzene was the 1 : 3 variety has since been established. The authors also made known the existence of a new tribromobenzene melting at 119°, the symmetric or 1 : 3 : 5-derivative. The isomerism of the liquid dibromobenzenes was subsequently conclusively demonstrated (*Ber.* 1874, vii. 1560) by the crystallographic examination of their mononitro-derivatives by Groth and Bodewig.

In this connection may be mentioned a short paper, in conjunction with Wurster, "On some derivatives of solid Dibromobenzene" (*Annalen*, 1874, clxxii. 57). In the attempt to prepare a nitrated phenylenediamine by acting upon dibromonitrobenzene with alcoholic ammonia, in the same manner that Walker and Zincke obtained nitraniline from monobromonitrobenzene, they found that only a moiety of the bromine could be displaced, the resultant product being a new substance, bromonitraniline, in which the NO_2 group is next to the NH_2 group. By converting this substance into bromophenylenediamine, the two NH_2 groups would be in close proximity; on debrominating this compound, the resulting phenylenediamine was found to be identical with that discovered by Griess (m. p. 99°), whose surmise that the NH_2 groups were united to contiguous carbon atoms was confirmed.

Meyer began also an inquiry on the chemical nature

of chloral hydrate, a substance which, in consequence of Liebreich's discovery of its anæsthetic action, had by that time become of considerable industrial importance, and was readily procurable. By the action of acetic chloride upon chloral hydrate, tetrachlorethyl acetate, $CCl_3 \cdot CHCl \cdot O \cdot CO \cdot CH_3$, is obtained, identical with the product formed by the condensation of chloral and acetic chloride, or by the action of acetic chloride on acetaldehyde. Chloral alcoholate with acetic chloride yields $CCl_3 \cdot CH\begin{matrix} OC_2H_5 \\ O \cdot CO \cdot CH_3 \end{matrix}$, indicating that the alcoholate, as already shown by Henry on other chemical grounds, is the ethyl ether of trichlorethylidene glycol. By dissolving chloral hydrate in glacial acetic acid, Meyer obtained a white, crystalline substance, melting at 80°, which he regarded as isomeric with ordinary chloral hydrate (m. p. 55°).

The chloral hydrates may have respectively the constitution

(1) $CCl_3 \cdot COH + H_2O$.
(2) $CCl_3 \cdot CH(OH)_2$.

It is not proved, however, that they are not polymeric (*Annalen*, 1874, clxxi. 65). It is possible that the substance thus obtained is identical with the uniaxial form observed by Pope which slowly changes into the biaxial modification stable at ordinary temperatures (*Trans.* 1899, lxxv. 458). Meyer does not definitely decide which of the two formulæ represents the constitution of ordinary chloral hydrate, but he inclines to regard it as trichlorethylidene glycol, which Perkin's observations on its magnetic rotation definitely indicate that it is.

Wallach's observation of the conversion of chloral into dichloracetic acid was shown by Meyer to be in

accordance with a general property of aldehydes in alkaline solutions to take up the elements of water, one molecule of aldehyde being thereby reduced, whilst another is oxidised.

Meyer, in conjunction with his pupil Haffter, devised a very simple and rapid method of estimating the actual quantity of pure chloral in the commercial product, founded on the fact that chloral hydrate is rapidly decomposed by an aqueous solution of an alkali into chloroform and an alkaline formate (*Ber.* 1873, vi. 600).

Although Meyer's papers up to this time had amply demonstrated his power of investigation, and afforded to critics like Kekulé and Baeyer abundant proof of the clearness and keenness of his vision, he had hitherto worked upon somewhat conventional lines. His memoir "On the Nitro-compounds of the Fatty Series" (*Annalen*, 1874, clxxi. 1), which appeared shortly after his removal from Stuttgart to Zürich, at once stamped him as an original investigator of a very high order. In anticipation that two series of alkyl nitrites would be found to exist, denoted in the methyl series by $CH_3 \cdot NO_2$ and $CH_3 \cdot O \cdot NO$, Meyer, whilst still at Stuttgart, had studied the action of amyl iodide upon silver nitrite and had obtained a colourless liquid, having the ordinary smell of amyl compounds, boiling between 150° and 160°, which on analysis was found to have the composition $C_5H_{11}NO_2$. Hence it was isomeric with amyl nitrite, a yellow liquid of a peculiar, disagreeable smell, which boils at 99°. To the new compound Meyer gave the name of nitropentane. The reaction was found to be general. He now entered upon an elaborate investigation of the nitro-compounds of the alkyl series and their derivatives, and of questions incidental to the

main subject, which, in conjunction with his pupils, among whom may be mentioned Stüber, Rilliet, Chojnacki, Wurster, Constam, Janny, Lecco, Locher, Tcherniac, Ceresole, Müller, Demuth, Keppler, and Züblin, continued to occupy him at intervals for upwards of twenty years.

One peculiarity which distinguishes aromatic from aliphatic compounds consists in the ease with which "nitration,"—that is, the replacement of hydrogen by the group NO_2 with the elimination of water—may be effected in the first-named substances. A reaction analogous to that by which nitrobenzene is formed from benzene was scarcely known among fatty compounds, the most familiar instance being the production of chloropicrin by the action of strong nitric acid upon chloroform. The constitutional difference between the nitroparaffins, as the new group came to be called, and the alkyl nitrites consisted in the fact that, in the first-named substances, the NO_2 group is directly connected with a carbon atom, as in nitrobenzene, whereas in the alkyl nitrites the NO group is intermediately linked with the hydrocarbon radicle by means of oxygen. The alkyl nitrites are true esters capable of being resolved into alcohols and nitrous acid by the hydrolytic action of alkalis or acids. The nitroparaffins, on the other hand, are incapable of hydrolysis. By reduction, they yield alkyl hydroxylamines and then the corresponding amine. By reduction, however, the alkyl nitrites lose their nitrogen and form the corresponding alcohol.

Meyer's view that the nitroparaffins were veritable nitro-compounds was not at once accepted. Geuther, and subsequently Götting, assumed that nitroethane was acetamidoxide, $CH_3 \cdot CO \cdot NH_2O$, whilst Kissel regarded it as acethydroxylamine, $CH_3 \cdot CO \cdot NH(OH)$.

It is true, as found by Meyer, that nitroethane, under the hydrolytic influence of strong acids, splits up into hydroxylamine and acetic acid, but since phosphoric chloride is without action upon nitroethane, an hydroxyl group must be assumed to be absent. As for Geuther's view of the constitution of these substances, Meyer had little difficulty in showing that it wholly failed to explain all their known reactions.

The general characteristics of the nitroparaffins, namely, the power of forming salts possessed by the primary and secondary compounds, the absence of this power in the tertiary series, together with the remarkable differences in the behaviour of bromine, and of the action of acids upon the primary and secondary nitroparaffins, were carefully studied by Meyer and his pupils. He found that when a solution of a primary nitroparaffin in potash is mixed with an alkaline nitrite and treated with sulphuric acid, the liquid acquires a blood-red colour which disappears on the further addition of acid. On shaking the whole with ether, there is obtained a solution of a new acid, known as a nitrolic acid, of the general formula $C_nH_{2n+1}\cdot C\begin{matrix}=N\cdot OH\\ \diagdown NO_2\end{matrix}$

In the case of nitroethane, the formation of the ethylnitrolic acid may be represented as follows:

$$CH_3\cdot C\begin{matrix}=H_2\\ \diagdown NO_2\end{matrix} + ON:OH = H_2O + CH_3\cdot C\begin{matrix}=N\cdot OH\\ \diagdown NO_2\end{matrix}.$$

That such is the constitution of ethylnitrolic acid is indicated by its formation by the action of hydroxylamine on dibromonitroethane,

$$CH_3\cdot C\begin{matrix}=Br_2\\ \diagdown NO_2\end{matrix} + H_2N\cdot OH = 2HBr + CH_3\cdot C\begin{matrix}=N\cdot OH\\ \diagdown NO_2\end{matrix}.$$

The nitrolic acids are colourless, sweet-tasting substances of a strong acid reaction, readily soluble in water, and

for the most part easily crystallisable. In alkaline solution they give an intense blood-red coloration and form characteristic precipitates with salts of the heavy metals. They slowly decompose on standing, and on heating are quickly resolved into the corresponding fatty acid, nitrogen, and nitrogen dioxide. On treating the nitrolic acids with sodium amalgam, substances known as azaurolic acids are formed. They are strongly coloured, sparingly soluble substances, and differ from the corresponding nitrolic acid by containing two atoms of oxygen less. The best known member of the series is ethylazaurolic acid, $C_2H_4N_2O$, or more probably $C_4H_8N_4O_2$. On heating, it yields, together with formation of nitrous oxide and water, ethylleucazone, $C_4H_7N_3O$, a substance possessing both acid and basic properties, and in its general characteristics resembling an amido-acid.

The secondary nitroparaffins, when treated with nascent nitrous acid, behave quite differently from the primary compounds. On adding sulphuric acid to the mixed solutions, a deep blue colour is produced and insoluble substances are formed, isomeric with the nitrolic acids, but which have no acid character. They were called by Meyer pseudonitroles, and have been regarded as nitrosonitro-compounds.

$$\begin{matrix} CH_3 \\ CH_3 \end{matrix}\!\!>\!C\!<\!\!\begin{matrix} NO \\ NO_2 \end{matrix}$$

Their formation may be thus represented

$$(CH_3)_2CH{\cdot}NO_2 + OH{\cdot}NO = H_2O + (CH_3)_2C(NO){\cdot}NO_2.$$

Scholl subsequently discovered that these substances may be obtained by the action of nitrogen peroxide upon the ketoximes: thus with acetoxime:

$$4(CH_3)_2C{:}N{\cdot}OH + 3N_2O_4 = 4(CH_3)_2C\!<\!\!\begin{matrix} NO \\ NO_2 \end{matrix} + 2H_2O + 2NO,$$

a mode of formation which, as Meyer pointed out,

indicated that they may be regarded as the nitrates of the oximes, $(CH_3)_2C{:}N{\cdot}O{\cdot}NO_2$. Their formation from the secondary nitro-compounds may be supposed to occur in the following phases:

$$(CH_3)_2CH{\cdot}N{<}\begin{matrix}O\\|\\O\end{matrix} + HNO_2 = (CH_3)_2CH{\cdot}N{<}^{O{\cdot}NO_2}_{OH},$$

$$(CH_3)_2CH{\cdot}N{<}^{O{\cdot}NO_2}_{OH} - H_2O = (CH_3)_2C{:}N{\cdot}O{\cdot}NO_2.$$

Meyer was inclined to give the preference to the latter view of their constitution, as it is generally very doubtful whether compounds containing a nitroso-group (NO) directly linked to a carbon atom are capable of existence.

Tertiary nitro-compounds are unchanged by the action of nascent nitrous acid.

Meyer pointed out how the characteristic colour reactions afforded by the behaviour of nitrous acid with the primary and secondary nitroparaffins, and its inability to act upon the tertiary compounds, offered a ready means of distinguishing primary, secondary, and tertiary alcohol radicles. The iodide to be tested is distilled with silver nitrite, the distillate shaken with a solution of potassium nitrite in strong potash, diluted with water, and mixed drop by drop with dilute sulphuric acid. If the liquid acquires a red colour (formation of nitrolic acid) which disappears with excess of acid and reappears on the addition of alkali, we are dealing with a primary radicle: should the liquid give a blue colour (formation of pseudonitrole) soluble in chloroform, the compound is derived from a secondary alcohol radicle; the non-formation of colour indicates a tertiary radicle. The test ceases to be of much practical value beyond the 5 carbon series (compare Meyer and Jacobson, *Lehrbuch der Organischen Chemie*, 1893, 253, *et seq.*)

The same line of inquiry was extended to the other main groups of aliphatic substances, and resulted in the discovery of new types of compounds. Thus, by the reduction of the isonitrosoketones and the isonitrosoacetoacetic esters, Meyer, in conjunction with Treadwell, obtained a series of volatile bases having apparently the generic formula $C_nH_{2n-4}N_2$, which they termed ketines and subsequently aldines. This group of substances is now generally known as the azines, and the substance first described by Meyer and Treadwell is dimethylpyrazine,

$$2CH_3{\cdot}CO{\cdot}CH{:}N{\cdot}OH + 6H = N\begin{<}\begin{matrix}C(CH_3){:}CH\\ \\ CH{:}C(CH_3)\end{matrix}\end{>}N + 4H_2O.$$

Meyer had the faculty of keeping more irons hot at a time than any man of his period. Although much of his thought and energy was directed in the first years of his sojourn in Zürich to the development of the new field of inquiry which his discovery of the nitroparaffins opened out, he continued his work on aromatic compounds, partly in defending positions he had already secured, and partly in breaking new ground. In the latter connection, reference may be made to his discovery, with Michler, of diazoxybenzoic acid, and to the new class of azo-compounds which he described in conjunction with Ambühl.

A point of some little interest at the time (1875) was his discovery that hydroxylamine and nitrous acid together yield nitrous oxide and water, $NH_3O + NO_2H = 2H_2O + N_2O$, in the same manner that nitrous acid and ammonia form nitrogen and water. The production of nitrous oxide by mixing together concentrated aqueous solutions of hydroxylamine sulphate and sodium nitrite constitutes a neat and striking lecture experiment. He

also showed, with Locher, that hydroxylamine may be obtained by a number of new reactions, as, for example, by acting on dinitropropane or ethylnitrolic acid with tin and dilute hydrochloric acid, when, in the one case, the aminé is liberated in conjunction with acetone, and, in the other, together with acetic acid.

(1) $CH_3{\cdot}C(NO_2)_2{\cdot}CH_3 + 8H = CH_3{\cdot}CO{\cdot}CH_3 + 2NH_3O + H_2O.$
(2) $CH_3{\cdot}C(N{\cdot}OH){\cdot}NO_2 + 4H + H_2O = CH_3{\cdot}CO_2H + 2NH_3O.$

These reactions showed that the rule, hitherto regarded as universally true, that nascent hydrogen reduces nitroxyl to amidogen, has its exceptions.

But perhaps the most important of Meyer's discoveries at this period was that of the oximes. He had observed that dibromonitroethane, under the action of hydroxylamine, passes into ethylnitrolic acid, and he anticipated that the analogous nitrosoacetone would be formed in like manner from unsymmetrical dichloracetone. Experiment showed, however, that the chlorine in dichloracetone was replaced by a hydroxylamine rest, whilst the ketonic oxygen was replaced by the oximido-group, forming a compound termed by Meyer acetoximic acid, but now known as methylglyoxime, $CH_3{\cdot}C(:N{\cdot}OH){\cdot}CH(:N{\cdot}OH)$. The fact that hydroxylamine would thus react upon carbonyl oxygen induced him, in conjunction with Janny, to study the action of this reagent upon ordinary ketones and aldehydes, and thus led to the discovery of the ketoximes and the aldoximes. This discovery has a twofold significance. The reaction not only serves to indicate the existence of carbonyl oxygen in compounds, and hence is of value as a mode of determining constitutional problems, but it brought into existence a number of substances yielding deriv-

atives of considerable interest. Further, it is not too much to say that the stereochemistry of nitrogen takes its rise from the discovery of the oximes.

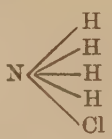
With Janny, he likewise obtained *a*-nitrosopropionic acid by the action of hydroxylamine on pyroracemic acid, a reaction which is almost quantitative and capable of being used as a test for pyroracemic acid.

Lossen discovered hydroxylamine as far back as 1865. Although it was quickly recognised as an exceedingly reactive substance, its use was greatly curtailed by the difficulty and expense of preparing it in quantity. Much of it, prior to 1883, was obtained by Dumreicher's process, namely, by reducing ethyl nitrate by means of stannous chloride and hydrochloric acid. Meyer showed how the irksomeness of the method, entailed by the necessity of removing the tin by sulphuretted hydrogen, and of dealing with the large volume of liquid produced, might be materially lightened, and considerable quantities of hydroxylamine salts were made by the modified process in the Zürich laboratory.

The position which hydroxylamine occupies between ammonia and nitric acid, which at that time were held to be the main nitrogenous foods of plants, as well as its great chemical activity when compared with the inertness of the other substances, seemed to Meyer to point to a possible formation of hydroxylamine within the plant, and to its playing an important part in the assimilation of starch and in the formation of albuminoids. In conjunction with Schulze, he therefore made comparative experiments on the action of hydroxylamine, ammoniacal salts, and nitrates upon plants, when it was quickly found that hydroxylamine acted as a poison to vegetable organisms. Meyer,

however, points out that it may still be possible that hydroxylamine may be formed in transition products, and yet act as a poison when taken up by the roots, just as peptone behaves as a poison when injected into the veins of animals.

Reference may here be made to Meyer's attempts to elucidate the constitution of ammonium salts. It was found that the dimethyldiethylammonium iodide, obtained by the action of ethyl iodide on dimethylamine, is identical with that produced by acting with methyl iodide on diethylamine, and no difference can be detected in the character of their salts. As the substances, although identical, were obtained by different reactions, it was inferred that they could not be "molecular" compounds, that is, combinations of a tertiary base with an alkyl haloid, but must contain pentavalent nitrogen, whence, by analogy, ammonium chloride would be



This assumption is only sound on the supposition that, in the formation of the salts, no change had taken place in the position of the alcohol radicles. The main conclusion would be invalidated if, for example, ethyl iodide, when reacting on trimethylamine, did not combine directly with it but was decomposed, as Lossen had suggested, as follows:

$$N(CH_3)_3 + C_2H_5I = CH_3I + N(CH_3)_2C_2H_5.$$

To ascertain if such an interchange occurred, Meyer, in conjunction with Lecco (*Annalen*, 1876, clxxx. 173), studied the action of ethyl iodide upon tetramethyl-

ammonium iodide. If Lossen's contention were sound, the reaction should be

$$N(CH_3)_4I + C_2H_5I = CH_3I + N(CH_3)_3C_2H_5I.$$

No action, however, was found to occur either with ethyl or methyl iodide alone at any temperature up to 180°, or in presence of methyl alcohol or water. Ladenburg and Struve tested Meyer's conclusion by making similar experiments with benzyl iodide and triethylamine, and with ethyl iodide and benzyldiethylamine, and were disposed to regard the resultant compounds as isomeric, although closely alike in most of their properties. On repeating the observations, Meyer found that no difference existed; the substances prepared in the two ways were absolutely identical.

Suggestive and fruitful in ideas as Meyer was, he was seldom at a loss in devising means to put them to the test of trial. In many of his mental characteristics not unlike Davy, as an experimentalist, he had all Davy's resourcefulness with far more than his patience.

As I knew him in Heidelberg, he was an excellent manipulator; still his temperament would never have permitted him to better the example of the great master under whom he was trained. We could all look on and marvel at the patient, concentrated power with which Bunsen would devise, elaborate, and perfect some new form of apparatus, or some new method of analysis. The first steps were very simple—so simple indeed that it was frequently impossible to divine their ultimate purpose. It was from such small beginnings that we obtained the whole process of gasometric analysis, the burner, the photometer, the various voltaic batteries, the spectroscope, the filter-pump, the ice-calorimeter, the flame reactions, etc. Before Bunsen gave a piece of

apparatus to the chemical world, he left it practically perfect; the striving after perfection was a veritable passion with him, and numerous were the forms or modifications through which the apparatus or the method passed before he rested satisfied with it. Although Meyer's genius was of a different order, the influence of the Heidelberg training is to be recognised in the various forms of laboratory apparatus with which his name is connected. Chief among these are his modes of determining vapour densities. The elegant modification of Gay Lussac's method introduced by Hofmann left nothing to be desired in the case of comparatively volatile substances unacted upon by mercury, but many bodies were known, and their number was being rapidly increased, in which this method was inapplicable. Meyer accordingly, in 1876, devised his displacement method (*Ber.* 1876, ix. 1216). This in principle was similar to the method suggested by W. Marshall Watts as far back as 1867, from which it differed in that Wood's metal—an alloy of bismuth, lead, tin, and cadmium, melting below 70°—replaced the mercury, and that the volatilisation was effected at the temperature of boiling sulphur, that is, at 444°. This process allowed of the determination of the vapour density of many substances which could be vaporised at temperatures below the boiling point of sulphur, and compounds like diphenyl, methylanthracene, triphenylamine, paradibromobenzene, and paradiphenylbenzene, had their vapour densities ascertained for the first time by means of it.

The method was further modified in the following year (*Ber.* 1877, x. 2068), mercury being used instead of Wood's metal and the vapours of boiling water, aniline, ethyl or amyl benzoate or diphenylamine—

depending on the temperature required—were employed as a bath instead of sulphur vapour.

The *Luftverdrängung Methode*—the simple and extremely convenient process—which will for all time be associated with the name of Victor Meyer, was devised in 1877 (*Ber.* 1877, xi. 1867). The apparatus is now one of the commonest articles of laboratory furniture, and it is not too much to say that, thanks to the ease with which the whole operation may be carried out, more vapour densities have been determined by its aid than by any other means.

The apparatus is usually constructed of glass, but by making it of glazed porcelain, determinations can be effected at very high temperatures. Except for special reasons, neither the temperature of the heated bulb nor its volume need be known: all that is required is that the temperature should be sufficiently high to gasify the substance under examination. A variety of liquids—water, xylene, aniline, ethyl benzoate, amyl benzoate, diphenylamine—depending on the temperature needed to effect complete vaporisation, may be used as media for heating the bulb. For temperatures exceeding 300°, a bath of molten lead may be employed, the glass bulb of the apparatus being coated, as suggested by Watson Smith and Davis, with a moderately thick film of soot before immersion in the bath so as to diminish the risk of fracture.

Mr. Watson Smith, who was with Meyer at Zürich, and who has kindly sent me some reminiscences of him at this period, writes: "It was somewhat singular that just as Victor Meyer, with Carl Meyer (no relation), had completed their vapour density apparatus for bodies of very high boiling point, I had just obtained in the pure state specimens of the three isomeric dinaphthyls,

all of which urgently awaited the determination of their vapour densities. They were the first new high boiling substances with which the apparatus and method were tried. Victor Meyer was immensely pleased and interested with this circumstance, and we practically all three worked the determination together, the results amply proving the reliability and accuracy of the new method. Of course in these cases the lead bath was used" (compare *Trans.* 1879, xxxv. 226; 1880, xxxvii. 491).

The molecular weights of a number of substances were quickly ascertained by this method, for example, phosphorus pentasulphide, indium chloride, cuprous chloride, stannous chloride, arsenious oxide, antimonous oxide, cadmium bromide, etc. Volatilised in an atmosphere of hydrogen chloride, ferrous chloride yielded values between $FeCl_2$ and Fe_2Cl_4. Ferric chloride at no temperature showed a vapour density corresponding with Fe_2Cl_6, whilst at 750° and 1077° its molecule would seem to be $FeCl_3$. Potassium iodide at 1320° in an atmosphere of nitrogen had a density corresponding with KI. Arsenic and phosphorus at a white heat had densities approaching the values for As_2 and P_2, whilst zinc at 1400,° and bismuth at 1700°, were found to be monatomic, and thallium at 1700° diatomic.

In 1879 the two Meyers, Victor and Carl, astonished the chemical world by announcing (*Ber.* 1879, xii. 1426), as the result of determinations of their vapour densities at high temperatures, that the halogens were capable of undergoing dissociation or possibly decomposition. As regards chlorine, this announcement at once threatened to re-open a question which had been regarded by most people as practically settled since 12th July 1810, when Davy read to the Royal Society his classical memoir on oxymuriatic acid. Davy, it is

true, had never stated that chlorine was an element in the absolute sense of that term. What he inferred was that it was a substance which "has not as yet been decompounded," and therefore is "elementary as far as our knowledge extends." The very name chlorine, which he suggested, inculcated this view. "To call a body which is not known to contain oxygen and which cannot contain muriatic acid, oxymuriatic acid, is contrary to the principles of that nomenclature in which it is adopted. . . . After consulting some of the most eminent philosophers in this country, it has been judged most proper to suggest a name founded upon one of its obvious and characteristic properties—its colour, and to call it *chlorine*, or *chloric gas*. Should it hereafter be discovered to be compound, and even to contain oxygen, this name can imply no error, and cannot necessarily require a change."

Had chlorine then been "decompounded"? Did it contain oxygen? Were Berzelius and Murray right after all? Was there such an entity as murium? The pages of the popular scientific periodicals of the time show how these questions agitated the minds of chemists. The indications of the spectroscope were advanced as confirmatory of Meyer's results, and there was much exercise of "the intelligent anticipation of events before they occur" which occasioned him some annoyance at the time. However sanguine he might be that he had decomposed chlorine, and however freely he might talk with his colleagues, he never committed himself in print to any statement of the kind. To begin with, the amount of oxygen he had obtained was very small, and there was uncertainty as to the action of the chlorine upon the silica or alumina of the porcelain at the high temperature, and whether the

materials employed were wholly free from moisture. I have it, on the authority of Professor Lunge, whose knowledge was derived from daily intercourse with him, that Meyer himself refused to consider the fact as established until he had worked in an apparatus made of material devoid of oxygen, and to this end he obtained a special grant from the Zürich authorities to defray the cost of a vessel of platinum.

Meanwhile Meyer's observations on chlorine were repeated, and their validity impugned by Crafts (*Compt. rend.* 1880, xc. 183 *et seq.*), who found, by a modification of Meyer's method, that the gas, even at the highest temperature of the Perrot furnace, showed no change indicative of dissociation or decomposition. Meyer, in conjunction with Züblin, at once repeated Crafts' determinations on pre-formed chlorine and confirmed their accuracy.

As regards bromine and iodine, however, the observers were in substantial agreement.

Thus with iodine:

Victor Meyer.			Crafts and Meier.		
Temp.	Density.	D'/D.	Temp.	Density.	D'/D.
450°	8·85	—	445°	8·74	—
586	8·72	0·99	830—880	8·07	0·92
842	6·76	0·77	1020—1050	7·01	0·80
1030	5·75	0·66	1275	5·82	0·66
1570	5·70	0·65	1390	5·28	0·60

Naumann showed from these results, on the assumption that the molecule I_2 splits up into two atoms $I + I$, that the course of the dissociation is in accordance with the result required by the mechanical theory of gases, namely, that the increments of decomposition corresponding to equal differences of temperature increase gradually from the temperature at which dissociation begins up to that at which 50 per cent of

the vapour is decomposed, and then decrease in a similar manner up to that temperature at which dissociation is complete.

In conjunction with Langer, Meyer greatly extended these observations, and subsequently published them as a monograph, entitled *Pyrochemische Untersuchungen* (Braunschweig: Vieweg u. Sohn, 1885).

As regards bromine, they found that the gas, when sufficiently diluted with air, had a normal density, namely 5·52 even at the ordinary temperature, and no sensible change occurred up to 900° even when diluted with eleven times its volume of nitrogen. At 1200° the density had diminished to 4·3 on dilution with five volumes of nitrogen. At a white heat the density of the diluted bromine fell to 3·9. Experiments at higher temperatures were not possible, as at above 1600° the platinum apparatus is rapidly attacked by both bromine and chlorine.

The alteration in density of bromine vapour at high temperatures has recently been studied by Dr. Perman and Mr. Atkinson, who have found that no sensible diminution occurs up to about 750° at atmospheric pressure, at which point dissociation becomes just appreciable, especially at low pressures, and gradually increases with increasing temperature (*Proc. Roy. Soc.* 1900, lxvi. 10).

In the case of chlorine, it was found by Meyer and Langer that no analogous change occurred below 1200°, no matter whether pure or highly diluted chlorine was employed. At 1400°, however, the density of the diluted chlorine fell from the normal value 2·45 to 2·02.

Similar observations with carbon monoxide seemed to show that at 1690° this gas is partially decomposed into carbon dioxide and carbon, $2CO = C + CO_2$. Carbon

dioxide itself experiences no change in density at this temperature in a platinum apparatus, although when passed through a porcelain tube filled with broken pieces of porcelain it undergoes dissociation, as already shown by Deville. This phenomenon may be connected with the remarkable observation of Menschutkin and Konowaloff that dissociable vapours are far more rapidly broken up in presence of asbestos, or pieces of glass, or even of the roughened sides of glass, than when the interior of the glass vessel is perfectly smooth.

Nitrous oxide is entirely resolved into oxygen and nitrogen at 900° when heated in a porcelain tube, and at 1690° when heated in a platinum tube. Nitric oxide remains unchanged up to 1200°; at 1690° it is completely decomposed into its elements. Hydrogen chloride also appears to be partially, whilst sulphuretted hydrogen is entirely, decomposed at the latter temperature. Cyanogen has a normal density up to 800°; at 1200°, it suffers decomposition.

Meyer made his pyrochemical investigations under very unfavourable conditions. The magnificent chemical institution which Zürich now possesses was not then built. The old laboratory was a low building to the east of the main block of the Polytechnicum, and the only room which could be spared for the purpose was so small that, in spite of the best ventilation possible, the temperature not unfrequently rose to 50°. Moreover, both he and his assistants suffered greatly from the strenuous ardour with which the work was carried on, and he himself eventually broke down under the strain of it. I saw him in Zürich in the autumn of this year (1879), and was surprised and shocked to notice, although it was at the end of the vacation, how nervous and jaded he seemed. I believe the distressing

insomnia from which he suffered at times throughout the rest of his life began at about this period. In reference to this time, Mr. Watson Smith writes: "Meyer had a most excitable mind and was a tremendous worker. His assistant, Carl Meyer, told me that on several occasions he was so overworked, not by compulsion, but through the mere influence of Meyer's presence, his mental power, and enthusiasm, that he came very near committing suicide during the fits of depression following exhaustion after long-continued spells of work."

Pyrochemical problems continued to interest Meyer to the end, and he was quick to take advantage of any hint which seemed to promise the possibility of their solution. In a lecture before the Naturforscher Versammlung in Heidelberg, he regretted that the lack of vessels of sufficiently refractory material prevented him from working at the higher limits of temperature even then attainable. "There can be no doubt," he said, "that new and undreamt-of discoveries will manifest themselves—that a new chemistry will disclose itself, when we are furnished with vessels that will enable us to work at temperatures at which water can no longer exist, and at which oxy-hydrogen gas becomes an uninflammable mixture."

Shortly before his death, he returned to the subject with new apparatus made of a platinum-iridium alloy capable of withstanding a far higher temperature than pure platinum, and he was in hopes of being able to construct vessels of magnesia which would allow of the application of temperatures over 2000°.

Meyer, at the time he announced his discovery of the dissociation of the halogens, was thirty-one years of age. He was now on the flood-tide of his prosperity.

His published work had shown him to be an investigator of uncommon power and originality, and students flocked to him from all parts, to participate in the pioneering work which his astonishing energy and enthusiasm opened out. In its triumphs it was indeed a time of "joyous yesterdays and confident to-morrows." He was happily married to the friend of his youth, Fraülein Hedwig Davidson, whose *Verlobungstag* was the very day on which he received his call to Zürich. What she was to Meyer only those who were privileged to know his home circle can fully realise.

Meyer's first great grief came to him with the death of his eldest daughter, and in 1882 he lost his friend Wilhelm Weith, Professor of Chemistry in the University. How close and intimate was their friendship was evident to all who frequented the meetings of the Zürich Chemische Gesellschaft, where the two professors were generally to be found seated side by side at the head of the table; it is reflected too in the obituary notice of his colleague which Meyer wrote for the *Berichte*.

In the autumn of 1882 Meyer was requested to undertake the delivery of the series of University lectures on benzene derivatives which had been interrupted by Weith's death. I have already attempted to indicate what Meyer was as a teacher. No one could possibly take greater pains in the preparation of his lectures, or study more to make them instructive and interesting. It is generally supposed that organic chemistry does not lend itself to effective lecture illustration. Such was not the case when Meyer had to teach it. "I well recollect," writes Mr. Watson Smith, "that the word most frequently used in Zürich in defining the opinions of

Victor Meyer's students of his lectures was 'brilliant'!" Another of our Fellows, Mr. John I. Watts, who attended his course in 1879-80, writes :—

What particularly struck me about his lectures was their finished style. He made fairly constant use of notes, speaking with great rapidity. Yet his treatment of the subject was very clear, and his language perfect. The experiments were always well prepared and exceptionally successful. Indeed, his lectures were most popular, and both at his work and outside the Polytechnic there was no professor who was more respected and admired by all students than Victor Meyer. Young, handsome, well-dressed—for a German professor,—with a quick wit and a genial manner, he was a welcome addition to any gathering. When, in 1881, he had a "call" to Aachen and elected to remain at Zürich, the students treated him to a torchlight procession and a grand *Kommers*. Meyer watched the *Fackelzug* of over 1000 students from a balcony, and later, sitting as the honoured guest in a still greater throng, he seemed indeed a happy man.

Similar testimony is given by Dr. Sudborough, who was with him in Heidelberg, and who writes :—

As a lecturer, Meyer was clear, concise, and extremely lucid, and his delivery was easy and natural. His lectures were given by the aid of carefully written notes, and were fully illustrated by experiments, the table being always crowded with apparatus both in the organic and inorganic lectures. It was very rare indeed for an experiment to fail; this was firstly due to Meyer's own dexterity as a manipulator, and also to the care which was bestowed upon the preparation of the experiments.

It was in the course of these lectures on benzene derivatives that Meyer came upon what is perhaps the most brilliant of all his discoveries—that of thiophen. How he lighted upon it is well known, but the story bears repetition. He desired to show his class the so-called indophenin reaction of Baeyer, at that time held to be indicative of benzene, but to his astonishment

not a trace of the characteristic blue colour made its appearance, although, as was his wont, he had rehearsed the experiment just prior to the lecture. It appeared that his assistant, Sandmeyer—himself one of Meyer's "discoveries"—had handed him a sample of the benzene made in the lecture course by heating benzoic acid with lime, and at once drew his attention to the fact that the rehearsal had been made with the ordinary laboratory supply—the *Benzol purissim. crystallisatum* of the dealers, and, of course, derived from coal-tar. Meyer, at the moment, was so fully occupied that he might well have put aside the incident, or have given no immediate heed to its significance. But that was not his way. Fortune scatters her chances indifferently, and every man may have his share, but it is not given to each to perceive when he is favoured, or to know when to grasp "the skirts of happy chance." Madame de Staël once said that a most interesting book might be written on the important consequences which spring from little differences, and it was the little difference that riveted itself on Meyer's mind. He at once began the investigation of the cause. All kinds of benzene to be found in Zürich were tested, and it was soon definitely established that it was only coal-tar benzene that gave the indophenin reaction. Meyer's first idea was that it might be occasioned by an isomeric—a second benzene found in coal-tar. Within less than a month he had ascertained that the reaction was due to some sulphuretted product accompanying coal-tar benzene, and that Baeyer's indophenin was probably a sulphur compound. Meyer's action was characteristic of him. Before communicating with Baeyer, he carefully repeated his experiments, and only when all ground for doubt was removed did he inform his friend

of his observations and of the inferences he had formed. Baeyer at once sent him a specimen of his indophenin, and the fact that it actually was a sulphuretted compound was then established. Meyer found that coal-tar benzene, after repeated shaking with oil of vitriol, no longer reacted with isatin, and hence he determined to search among the sulphonic acids so formed for the reactive substance. By distilling the product obtained by shaking 10 litres of benzene with oil of vitriol, he obtained a few cubic centimetres of a clear, thin, mobile liquid containing sulphur, which boiled at about 83° and remained liquid in a freezing mixture of ice and salt. It gave a most intense reaction for indophenin. The amount of the new substance present in coal-tar benzene was very small, at most not more than 0·5 per cent.

Thanks to the co-operation of his friends, Messrs. Bindschedler, Busch and Co., of Basle, he was enabled to repeat this experiment on a large scale, as much as 250 litres of coal-tar benzene being operated upon at a time, and the sulphonic acids converted into the lead salts, which were then mixed with sal ammoniac and distilled. The crude product, which contained about 28 per cent of sulphur, was found to react strongly with bromine, forming a heavy, colourless, highly refractive liquid, boiling at 211°, greatly resembling dibromobenzene, but showing on analysis the composition $C_4H_2Br_2S$. The new compound was the dibromo-substitution product of a substance which Meyer was at first inclined to call *thianthren*, then *thiophan*, next *thiol*, and lastly *thiophen*, to denote that it was a sulphur-containing substance giving derivatives analogous to those of phenyl.

In the early part of the following June, that is,

within about six months of his first observation, he had obtained a considerable quantity of thiophen, and was in a position to show to the Swiss Naturforscher Versammlung, which met during that summer in Zürich, that its chemistry was hardly less extensive than that of benzene itself. Thus it was that a chance observation —the observation of a little difference—added a new section to organic chemistry.

It would be quite impossible within the limits at my disposal to show how this section was developed by Meyer and his pupils. In 1888 he published, in the form of a monograph, dedicated to his friend and patron, President Kappeler, *dem hochherzigen Förderer wissenschaftlicher Bestrebungen*, an account of its condition at that time (*Die Thiophengruppe.* Braunschweig: Vieweg u. Sohn), from which it appears that during the preceding five years no fewer than 106 contributions to its history had been made from his own laboratory, and some 40 from those of others. How fertile the field still continues may be gleaned from the circumstance that upwards of 50 papers on the same subject have since made their appearance in various journals.

Meyer's restless energy had now begun to react most seriously upon his general health. At times he was almost prostrated by nervous exhaustion; he had frequent spells of insomnia, and his friends viewed with alarm the signs of physical decay which now set in, in spite of the occasional holidays, mainly among the Alps, which he gave himself. In 1884 he again broke down, and although at the moment he was immersed, with his colleague Lunge, in the plans for the new chemical laboratory of the Polytechnic in which, as he states, he had hoped ultimately to resume his pyrochemical labours under more favourable conditions, he was obliged to

relinquish, for a time, all idea of work, and towards the end of the year was ordered away to the Riviera, where he wintered. In the following spring he received a "call" to Göttingen as the successor of Hübner. This he eventually decided to accept, and entered upon his duties there in the summer session of 1885.

He was not able, as he had hoped, to take leave of his Zürich students at the time, but what they thought of him—with what affection and respect he was regarded—was seen in the terms of the Address they sent to him on the occasion of his opening lecture at Göttingen. It was seen, too, in the way he was received by them during a visit to Zürich some months later, on the occasion of the seventieth birthday of his friend Kappeler. Professor Goldschmidt, who was present, thus describes the scene: "I see him even now before me as he spoke to the students at the *Kommers* in the evening. The 'Zürcher Polytechnikers' have, as a rule, but little opportunity of knowing the professors outside their special faculty, and have, therefore, but little interest in those who are not their own teachers. As Victor Meyer's slender form appeared on the platform, and as his bright blue eyes glanced round the assembly, there broke forth a shout of welcome from all—engineers, machinists, architects, as well as from his old students the chemists—to be ended in a whirlwind of applause at the close of a speech, sparkling and witty as ever."

With renewed health and vigour, he now set about the plans for the new laboratory which the authorities had decreed should adorn the "Georgia-Augusta," for Wöhler's old place no longer sufficed to contain the chemical workers which Göttingen had now to receive. Whilst it was being erected, he continued his pyro-

chemical work, and his investigation of the thiophen derivatives, and began with Paul Jacobson the admirable *Text-book of Organic Chemistry*, which, in the critical selection and arrangement of its material, is still unsurpassed.

With Auwers, he resumed the study of benzil and its derivatives which he had begun with Wittenberg and Goldschmidt. With Münchmeyer, he began the study of the behaviour of phenylhydrazine towards various groups of oxygen compounds, and he sent a short paper to the *Berichte* on thiodiglycol compounds, and on an easy method of preparing β-iodopropionic acid from glycerol. With Demuth, he undertook the investigation of the sulphuranes, a group of disulphides of the general formula $C_2H_3 \cdot S \cdot C_2H_4 \cdot SR$. Other papers were on the density of nitric oxide, which showed no evidence of association or molecular duplication even at -100°; on isophthalaldehyde; on the negative nature of the phenyl group; and on isodibromosuccinic acid.

It was characteristic of his receptivity that Meyer should be among the earliest workers in Germany to perceive the value of Raoult's method of ascertaining molecular weights; it was first used in the Göttingen laboratory to determine the molecular weight of some derivatives of benzil which yielded two series of isomeric compounds, both series having the same constitution, in the ordinary sense, but which were yet distinct from one another, and yielded different derivatives.

Other papers of this period were on the thio-derivatives of deoxybenzoin and its analogues (desaurins), and, with Riecke, on the carbon atom and valency. The latter paper is of interest as an example of Meyer's "scientific use of the imagination," and may be studied in connection with an earlier paper in 1876 (*Annalen*,

1876, clxxx. 192) on the same subject, as showing how he grafted the theoretical conceptions of van't Hoff upon the teaching of Kekulé. According to Meyer, the carbon atom is surrounded by an ethereal shell which, in the case of an isolated atom, has a spherical form; the atom itself is the carrier of the specific affinities, the surface of the shell is the seat of the valencies; each affinity is determined by the existence of two opposite electrical poles, which are situated at the end-points of a straight line small in comparison with the diameter of the ethereal shell. Such a system of two electric poles is called a double- or di-pole. The four valencies of a carbon atom would be represented by four such di-poles, the middle points of which are situated on the surface of the ethereal shell, but freely movable within it. The di-poles themselves can rotate freely round their middle point. The carbon atom has a greater attraction for positive than for negative electricity, and the positive pole of a valency is slightly stronger than the negative pole. This hypothesis explains why the four valencies take up the position of a regular tetrahedron; why they can be diverted from this position; why the valencies of one and the same carbon atom cannot combine together, whilst the valencies of different carbon atoms can do so; why there are two kinds of single-binding, one stable, and the other allowing free rotation; and lastly, why free rotation ceases in cases of double- or treble-binding (*Ber.* 1888, xxi. 946; compare Abstr. 1888, 549).

Stereochemical questions—we owe the phrase to Meyer—were indeed at this time occupying much of his thought. In a paper with Auwers (*Ber.* 1888, xxi. 784), he pointed out that the existence of the two isomeric dioximes of benzil, discovered by him

in conjunction with Goldschmidt, would, if the formula

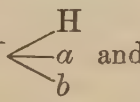
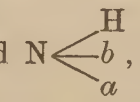
$$C_6H_5\cdot C(N\cdot OH)\cdot C(N\cdot OH)\cdot C_6H_5$$

were established for both, be in direct contradiction to the hypothesis of van't Hoff that two carbon atoms united by a single affinity are free to rotate, the axis of rotation being the bond of union, and that isomerism is only possible for those substances of the type $\equiv C—C\equiv$, which cannot, by rotation round the common axis, be converted into the same form. The two dioximes were carefully compared as regards their melting points, solubilities in water, alcohol, ether, or acetic acid, and the conditions under which the *α*-form is converted into the *β*-modification were ascertained. To further remove all doubt as to the possibility of merely physical isomerism, and to prove that the oximes are not only different from one another, but yield different derivatives reconvertible into their respective oximes, the propionic and isobutyric derivatives were prepared and compared. The result showed that the dioximes were of identical chemical composition, and hence it appeared that van't Hoff's hypothesis must be so altered as to admit of cases in which free rotation round the axis cannot take place, as otherwise no explanation of the isomerism of the *α*- and *β*-dioximes is possible (compare Abstr. 1888, 597).

Meyer subsequently showed how the work of Bethmann, Graebe, and Baeyer confirmed these views. Wislicenus' theory as to the free rotation of singly-bound carbon atoms would appear to be limited to certain cases; absolutely free rotation is probably possible only when the substituting atoms or groups are identical; where, as is the case in the majority of compounds, the atoms or groups are not identical, there will be some definite

position of equilibrium; only in cases where the substituting atoms or groups are equally negative will there be several positions of equilibrium (*Ber.* 1890, xxiii. 2079; Abstr., 1890, 1083).

Meyer's perspicacity and critical insight are well illustrated in a lecture which he gave to the German Chemical Society in 1890 "On the Results and Aims of Stereochemical Research." It is of interest to the student as giving a fairly complete historical account of the development of space formulæ, and more especially for its criticism of the work of Baeyer and Wislicenus on the stereochemical formulæ of single-, double-, and treble-linked carbon compounds, and of the stereochemical conceptions of Hantzsch and Werner in the case of nitrogen compounds.

With regard to the assumption that the nitrogen atom may be represented as a tetrahedron, and that the isomerism of the benzaldoximes may be similar to that of fumaric and maleic acids, it is pointed out that the structure of the oximes is in all probability not identical. Two isomeric forms of each of the unsymmetrical oximes of the formula OH—N:CXY are indicated by the hypothesis of Hantzsch and Werner, but they do not appear to exist. If the tetrahedral representation of the nitrogen atom were tenable, we should have to assume that substituted ammonias can exist in the isomeric forms  and  , but such bodies are not known. We must therefore assume that in ammonia the hydrogen atoms are placed symmetrically with regard to the nitrogen atom, and this can only find expression in a plane formula (*Ber.* 1890, xxiii. 567; compare Abstr. 1890, 719).

Reference may be made here to the short paper on isomeric oximes of unsymmetrical ketones and the configuration of hydroxylamine, in conjunction with Auwers (*Ber.* 1890, xxiii. 2403), in which the authors advance further evidence that the isomerism of the oximes cannot depend upon structural dissimilarity, but must be sought for in the nature of the hydroxylamine group. Assuming the correctness of the theories of van't Hoff and Wislicenus regarding the arrangement of atoms in space, the combined effect of the attraction of the nitrogen and oxygen on the hydroxylic hydrogen of hydroxylamine would cause it to be in a plane different from that occupied by the remaining atoms in the molecule. The hypothesis suffices to explain all observed facts: unsymmetrical oximes would therefore exist in two forms, $\overset{a}{\underset{b}{C}}:N\cdot\overset{H}{O}$ and $\overset{a}{\underset{b}{C}}:N\cdot\underset{H}{O}$. The difference between this theory and that of Hantzsch and Werner is shown by the two formulæ,

$\overset{OH}{C}:N$ (Hantzsch and Werner). and $\overset{H}{C}:N—O$ (Auwers and Meyer).

The formation of an oxime by the action of nitrous acid is readily accounted for on the ground that it is a substituted hydroxylamine; moreover, the fact that no case of geometrical isomerism has ever been observed in the azo-, azoxy-, and imido-compounds tells in favour of this theory (compare Abstr. 1890, 1263).

To this period belongs also the work on the azines; on deoxybenzoin; on the aromatic nitriles; on tetramethylsuccinic acid; and on the oximes of phenanthraquinone.

Meyer was not destined to remain long in Göttingen. The new laboratory was barely finished, when, in 1889, he received a "call" to Heidelberg. Bunsen, full of years as of honours—the Nestor of Chemistry, as his friends were wont to call him—had expressed a wish to retire, and of all his many students there was none, he said, whom he wished more to take his place than he who, twenty-one years before, had worked with him in the modest little room of some four or five places, which had constituted his private laboratory. To Heidelberg accordingly Meyer went, with the coveted title of Geheimrath, and the promise of a new and enlarged laboratory. Although only forty years of age, he was now, so far as worldly position was concerned, at the summit of his career; he had returned to his Alma Mater, and the rest of his days were to be given to her service.

During his four years' stay in Göttingen he had in great measure recovered his health and with it the elasticity of his active, buoyant temperament. I saw him in Heidelberg in the spring of 1891, when he was busy with the enlargement of the old laboratory, and it was with a glance of pride—a pardonable pride—that he pointed out the places where he and I had worked with "Papa" Bunsen,

> So kindly modest, all accomplished, wise,

in the corner place near the window, towering above both of us. It was strange, too, to hear the sound of children's voices and their laughter, and the bustle of servants in what were formerly the silent, half-deserted rooms overlooking the Wredeplatz; and stranger still to me was it, as we together called upon Bunsen, sitting solitarily in his rooms overlooking the Bunsenstrasse,

to behold the meeting and to listen to the greeting of these two men—the memory of whose names and fame Heidelberg will cherish so long as Heidelberg exists. To all of us life has its dramas, and in some of these the incidents are as moving as those ever conjured up by playwright or poet.

This was the last occasion on which I saw Meyer. How well I remember it! He was as bright, as active, as mentally vigorous as of old, although it was but too obvious that his physical strength was not the equal of his nervous energy.

Meyer's earliest experimental work at Heidelberg was mainly concerned with the continuation of investigations begun at Göttingen. But he was perpetually breaking new ground or seeking to clear up doubtful points in ground already partially explored.

The classical labours of Frankland on zinc ethyl might be thought to have definitely established the chemistry of that substance, but even on such a comparatively simple matter as the action of oxygen on zinc ethyl there was room for still further inquiry. The white compound obtained by Frankland, as the result of the oxidation of zinc ethyl, was regarded by him as a mixture of zinc oxide, ethoxide, and acetate. It was found, however, by Meyer and Demuth to contain no acetate, but to be mainly composed of a peroxide, $ZnEt{\cdot}O{\cdot}OEt$, as proved by its power of liberating iodine from potassium iodide. The explosive character of the substance is thus explained. Zinc ethoxide, in fact, does not appear to have been prepared (*Ber.* 1890, xxiii. 394).

In conjunction with a number of his pupils—Krause, Freyer, Askenasy, and others—Meyer in 1891 began the investigation of a subject already associated with

the name of his predecessor, namely, on the conditions determining both the gradual and the explosive combustion of gaseous mixtures. Although a considerable amount of experimental work was done, the results obtained, curious and interesting as they were in some particulars, led to no very definite general conclusions.

It was found that the temperature at which combination occurred varied with the nature of the vessel, and depended upon whether the gases were confined or not. Ignition takes place at a lower temperature when the mixture is in a closed vessel than when passing freely through an open tube. If, however, an open vessel containing the mixture is heated suddenly, explosion takes place at a lower temperature. In the cases of gradual union, no relation between time and amount of combination could be perceived. As showing the influence of the nature of the surface, it was found that, when the bulbs were silvered inside, the union of oxygen and hydrogen was rapidly effected at a temperature of about 200°, whereas in an unsilvered bulb the gaseous mixture had to be heated to above 530° before any sensible amount of water was produced. The principal quantitative results are embodied in the following table in which the mixture did not explode at the lower temperature in each column, but did so at the higher :—

Equivalent mixtures.	Free current.	Closed bulbs.
Hydrogen, oxygen . . .	650—730°	530—606°
Methane, oxygen	650—730	606—650
Ethane, oxygen	606—650	530—606
Ethylene, oxygen . . .	606—650	530—606
Carbon monoxide, oxygen . .	650—730	650—730
Hydrogen sulphide, oxygen . .	315—320	250—270
Hydrogen, chlorine . . .	430—440	240—270

In a subsequent paper with Münch (*Ber.* 1893, xxvi. 2421), the temperatures of explosion of gaseous mixtures

were determined by placing the vessel containing the gases inside the bulb of an air thermometer immersed in a metal bath. The mixture of a gas with the amount of oxygen theoretically necessary for its complete combustion was passed through a fine tube to the bottom of the internal vessel, and lighted as it issued from the mouth of the exit tube. At a certain temperature, the flame ran down the tube and the contents of the vessel exploded. This temperature—the temperature of explosion—was determined by displacing the air contained in the air thermometer by means of hydrogen chloride, collecting it over water, and measuring it. In thirty-eight experiments with a mixture of hydrogen and oxygen (pure electrolytic gas), the temperature of explosion varied from 620° to 680°, being about 650° in mean. The temperature is not affected by variations in the rapidity with which the gaseous mixture enters the glass vessel, or by the presence of fragments of glass or sand. In presence of platinum, the gases combine quietly, and if the glass vessel is very small no explosion occurs.

The temperatures of explosion of a number of aliphatic hydrocarbons, mixed with equivalent amounts of oxygen, were then determined as follows :—

Methane .	656—678°	Propane .	545—548°
Ethane .	605—622	Propylene .	497—511
Ethylene .	577—590	*iso*Butane .	545—550
Acetylene .	509—515	*iso*Butylene .	537—548
Coal-gas with 3 times its volume of oxygen . .			647—649

It would thus appear that the temperature of explosion falls as the number of the carbon atoms in the molecule increases; that it is probably lower for primary than for corresponding secondary hydrocarbons; and is less for hydrocarbons containing a double bond

than for those containing only single bonds, and still less for those containing a triple bond (compare Abstr. 1894, ii. 11).

Mention may here be made of the work done, in conjunction with Bodenstein, on the decomposition of gaseous hydrogen iodide by heat. This gas was selected for the reason that the action of heat upon it is reversible, and hence it might be expected that the establishment of a condition of equilibrium will be in no way influenced by the many disturbing circumstances which were found to occur in the case of other gaseous mixtures.

Combination of hydrogen and iodine vapour takes place even at 444° (b. p. of sulphur), and the hydrogen iodide formed is far more stable, at all events in the dark, than has hitherto been supposed. It is, however, very sensitive to light. In bulbs which were exposed for ten days to direct sunshine, 58 per cent of the gas was decomposed, and when exposed throughout the summer, practically the whole of the gas is resolved into its constituents. Experiments on the relation of the amount of decomposition to temperature gave the following results :—

Temperature of boiling.		Relative amount of HI decomposed, as determined	
		By decomposition.	By direct union.
Sulphur . .	444°	0·2150	0·2104
Retene . . .	394	0·1957	—
Mercury . .	350	0·1731	0·1738

At 310° (b. p. of diphenylamine) the relative amount of HI decomposed was 0·1669, instead of 0·1550 as calculated from the above numbers. The difference between the observed and calculated results is due to the circumstance that the heat of formation of hydrogen iodide is, at ordinary temperatures, negative

(−1600°, Thomsen), but from the fact that the decomposition at temperatures such as 350—444° increases with rising temperatures, it follows from van 't Hoff's principle (*Principe de l'équilibre mobile*) that the heat of formation at these temperatures is positive. There must, therefore, be a temperature at which the heat of formation is zero, and at which also the decomposition is at a minimum. This point, as the experiments show, lies between 310° and 350°, and calculation by van 't Hoff's formula showed that it is at 324°. As it was found that two bulbs heated under the same conditions always gave the same result, it was possible to study the decomposition as a time reaction, and by the formula given by Nernst. The constancy in the values actually obtained for each of the foregoing temperatures showed that, in the case of the decomposition of hydrogen iodide by heat, the change occurs in a perfectly regular manner (*Ber.* 1893, xxvi. 1146; compare Abstr. 1893, ii. 369).

This short account of Meyer's labours in physical chemistry may conclude with a brief reference to the determinations of the fusing points of salts melting only at relatively high temperatures, which he made in concert with his pupils. These he was able to obtain by the aid of the platinum air-thermometer he described in conjunction with Freyer. The following is a list of his final values :—

Sodium chloride	815°	Sodium biborate	878°
Sodium bromide	757	Sodium sulphate	863
Sodium iodide	661	Potassium sulphate	1078
Potassium chloride	800	Cæsium iodide	621
Potassium bromide	722	Calcium chloride	806
Potassium iodide	684	Strontium chloride	832
Potassium carbonate	878	Barium chloride	921
Sodium carbonate	849	Rubidium iodide	641

(Compare Heycock and Neville, *Trans.* 1895, lxvii. 190).

In 1892 Meyer and Wachter made known the possibility of the existence of a class of aromatic derivatives, known as the iodoso-compounds, in which the monovalent group IO replaces hydrogen. The first representative of the series was iodosobenzoic acid, $C_7H_5IO_3$, which they obtained by the action of fuming nitric acid or a boiling and acidified solution of potassium permanganate upon orthoiodobenzoic acid. It is a crystalline, solid substance, melting at about 200°, sparingly soluble in water or ether. It liberates iodine from potassium iodide, and chlorine from hydrochloric acid, forming orthoiodobenzoic acid. It is an extremely feeble acid, and its silver salt when dry is highly explosive.

No iodoso-derivatives could be obtained from meta- or para-iodobenzoic acids. Of the two iodoparatoluic acids, the one in which the iodine atom occupies the ortho-position to the carboxyl group yields an iodoso-derivative similar to iodosobenzoic acid, but the isomeric acid does not yield a corresponding compound. If, however, paraiodobenzoic acid be previously nitrated, it may be converted by the further action of fuming nitric acid into an iodoso-derivative, $IO{\cdot}C_6H_3(NO_2){\cdot}CO_2H$. In like manner, the iodoparatoluic acid may be made to yield an iodoso-compound by previous nitration (*Ber.* 1893, xxvi. 1354).

By the further action of oxygen on an alkaline solution of iodosobenzoic acid, iodoxybenzoic acid, $IO_2{\cdot}C_6H_4{\cdot}CO_2H$, is formed, a white, crystalline substance, turning red on exposure to light, and decomposing with explosion at 233°. It is a much stronger acid than the iodoso-derivative, forms moderately stable salts, and gives characteristic colour reactions with aniline and phenol (*Ber.* 1893, xxvi. 1727; compare Abstr. 1893, i. 577).

Hartmann and Meyer found that when iodosobenzene, $C_6H_5 \cdot IO$, is dissolved in strong sulphuric acid, the solution, on dilution with water, yields the sulphate of a base, phenyliodophenyliodonium hydroxide,

$$\begin{matrix} C_6H_5 \\ I \cdot C_6H_4 \end{matrix} \rangle I \cdot OH.$$

A similar change occurs with paraiodosotoluene. The free bases have a strong alkaline reaction, and form characteristic crystalline salts.

Meyer was thus led to the discovery of a remarkable group of substances known as the iodonium compounds, the simplest aromatic representative of which is diphenyliodonium hydroxide,

$$I \begin{matrix} C_6H_5. \\ C_6H_5. \\ OH. \end{matrix}$$

These bases are compounds in which two of the valencies of the iodine atom are satisfied by aromatic radicles, whilst the third is satisfied in the free base by hydroxyl, and in the salts by an acid radicle. The iodonium bases are readily soluble in water, are strongly alkaline, and in their behaviour, as in that of their salts, show a remarkable similarity with the derivatives of silver, lead, and, more particularly, thallium. These bases are formed by the decomposition of the iodoso- and iodo-hydrocarbons under various conditions; for example, by the action of moist silver oxide upon an intimate mixture of equivalent proportions of iodosobenzene and iodoxybenzene,

$$C_6H_5 \cdot IO + IO_2 \cdot C_6H_5 + AgOH = C_6H_5 \cdot I \begin{matrix} OH \\ C_6H_5 \end{matrix} + AgIO_3.$$

By the addition of potassium iodide to the aqueous solution, diphenyliodonium iodide is precipitated. This compound stands in the same relation to iodobenzene that trimethylsulphonium iodide does to methyl sul-

phide, and as tetramethylammonium iodide does to trimethylamine. It crystallises from alcohol in long, pale yellow needles, and decomposes on heating almost quantitatively into iodobenzene,

$$C_{12}H_{10}I_2 = 2C_6H_5I.$$

If the decomposition is started at one point, it proceeds through the whole mass with development of heat.

The existence of these remarkable bases and salts, which recall the sulphonium-, ammonium-, arsonium-compounds, etc., shows that a complex which is composed of one iodine atom and two molecules of phenyl, that is, of constituents which are otherwise negative, possesses strongly basic properties. From the general behaviour of the iodonium compounds, it is evident that the complex $-I\langle^{C_6H_5}_{C_6H_5}$ or in general $-I\langle^{R}_{R_1}$ (where R and R_1 are aromatic radicles) possesses the function of a metal analogous to thallium (compare *Lehrbuch der organischen Chemie*, Meyer and Jacobson, ii. 127).

It is interesting to note that the physiological action of the diphenyliodonium chloride resembles that of ammonium salts on the one hand, and of heavy metals, such as lead and thallium, on the other. Doses of 0·02—0·03 gram produce total paralysis in frogs, both the motor nerve-ending and the muscle substance being affected. A dose of 0·08 per kilo proves fatal to rabbits, the spinal cord and *medulla oblongata* being also affected.

A study of the conditions determining the formation and hydrolysis of ethereal salts of aromatic acids occupied Meyer, in conjunction with his pupils, and more especially Sudborough, from 1894 up to the year

of his death. It was found that benzoic acid and its substituted products, as a rule, readily yield practically the theoretical quantity of an ethereal salt when treated with methyl alcohol and hydrochloric acid in the cold. On the other hand, the symmetrical trisubstitution products of benzoic acid yield no ethereal salts whatever under these conditions. This rule holds absolutely for all 1 : 3 : 5-trisubstitution derivatives of benzoic acid (Me, No_2, Cl, Br, I, and CO_2H), except those containing one or more hydroxyl groups. The same is true of all substituted benzoic acids in which the 2 : 6-hydrogen atoms ($CO_2H = 1$) have been replaced by other atoms or groups.

The acids which do not yield ethereal salts when treated with alcohol and hydrochloric acid can readily be converted into these substances by the action of methyl iodide on their silver salts, or of methyl alcohol on the acid chlorides.

This remarkable difference in behaviour may be ascribed to a stereochemical cause, the substituent groups being supposed to hinder the introduction of the alkyl group to such an extent that under the prescribed conditions the reaction does not proceed.[1] Acids in which the carboxyl group is linked with the benzene nucleus by one or more carbon atoms readily undergo etherification. The constitution of a substituted benzoic acid may, therefore, be ascertained in this way, and the method may also be used for isolating or purifying those acids which will not undergo etherification.

The nitrophthalic acids behave similarly : dinitroph-

[1] This supposition has been modified by Wegscheider, who introduces Henry's conception of the formation of an additive compound between the alcohol and the acid. According to Wegscheider, the ortho-substituents prevent the formation of such additive compounds (compare, however, Davis, *Trans.* 1900, lxxvii. 33).

thalic acid [$NO_2 : CO_2H : CO_2H : NO_2 = 1 : 2 : 3 : 4$] gives no ethereal salt, whilst the acid [$NO_2 : CO_2H : CO_2H : NO_2 = 1 : 3 : 4 : 5$] yields monalkyl salts. 1 : 2 : 6-Dinitrobenzoic acid gives no ethereal salt (Abstr. 1895, i. 93). Chloronitrobenzoic acid [$CO_2H : NO_2 : Cl = 1 : 2 : 6$] yields no ethereal salt, showing that the rule applies when the substituents are dissimilar. Diortho-substituted benzoic acids are not etherified at low temperatures when one of the substituents is hydroxyl.

Meyer was of opinion that etherification, in the case of analogous compounds, is diversely influenced by substituents of different relative mass. He imagined that those radicles which prevent etherification at high temperatures have a much greater relative mass than those which only hinder it at low temperatures, but it is probable that the methyl group and its normal homologues will produce almost identical effects, since the action is chiefly due to that carbon atom which is directly linked to the benzene nucleus. According to theory, those ethereal salts which are formed with greatest difficulty should be hydrolysed least readily, and such Meyer found to be generally the case.[1]

These conclusions have been tested by Kellas (*Zeit. phys. Chem.* 1897, xxiv. 221), who has measured the velocity of etherification for a large number of monosubstitution derivatives of benzoic acid. From a study of the influence of temperature, and of the nature of the substituent, he has more precisely indicated the limits within which they may be regarded as generally true.

Considerations of space preclude more than a bare reference to the work on the derivatives of ethyl

[1] This is approximately true for the benzoic series of acids, but does not obtain for the acetic and probably other series (compare Sudborough and Lloyd, *Trans.* 1899, lxxv. 467).

dinitrophenylacetate; on the indoxazen group; on the laws of substitution in the aliphatic series; on the synthesis of triphenylacrylonitrile and on the isomerides of triphenylacrylic acid; on the modes of introduction of acetyl groups into aromatic hydrocarbons; on the substitution of the hydrogen in trinitrobenzene by alkaline metals; on the fusibility of platinum; on the formation of tetriodoethylene from diiodoacetylene; on the durenecarboxylic acids; on the action of potassium permanganate on hydrogen, methane, and carbon monoxide; on the slow oxidation of hydrogen and carbon; on the evolution of oxygen during reduction; on hexahydrobenzophenone and its oximes; and on diphenylamine from orthobromobenzoic acid.

Meyer contributed to the literature of chemistry, either alone or in conjunction with his pupils, upwards of 300 memoirs and papers.

No account of Meyer's scientific activity would be complete without some allusion to the various pieces of apparatus with which he enriched operative chemistry. Reference has already been made to his methods of determining vapour density, and to his mode of ascertaining the melting points of substances fusible only at high temperatures. He also greatly improved the methods of accurately determining the solubilities of substances at various temperatures. His form of water-bath is to be met with in many modern laboratories, surmounted, it may be, with the funnel-shaped cover which he devised to prevent access of dust to the evaporating liquid. Particularly neat and convenient are the drying ovens he constructed in which constant temperatures are obtained by means of liquids of different boiling points, for example, toluene, xylene, anisoil, etc.

Analytical chemistry had little attraction for Meyer, and, beyond his node of diagnosing primary, secondary, and tertiary alcohols and alcoholic radicles by colour reactions (see p. 440); and the method he devised, with Jannasch, for the simultaneous determination of carbon, hydrogen, and nitrogen in the elementary analysis of organic substances, he made no contribution to this department of the science.

As the director of a large chemical laboratory, and as a laboratory teacher, Meyer worthily followed in the footsteps of Bunsen. In proof of this, I may here quote the testimony of some of our Fellows who have worked under him.

Mr. John I. Watts thus refers to him in the Zürich days :—

> Victor Meyer as a teacher had a wonderful faculty of infusing enthusiasm into his students. He was constantly in the laboratory, and whether the pupil was engaged upon the analysis of some simple, well-known substance, or was pursuing original investigation, he seemed somehow to succeed in making him feel how interesting was his work. Possessed of a very quick and active intelligence, he would point out the reason of the difficulties almost before the student had finished recounting them. He was himself a constant worker, and when engaged in his work he always appeared to be in a high state of pleasurable excitement.

Similar testimony is afforded by English chemists who were with him at Heidelberg. Dr. J. T. Hewitt writes :—

> Professor Meyer was universally liked by the men who had the privilege of working under him. He had an extraordinary capacity for hard work, and his example, together with the interest he took in his men, induced in them a more or less similar love of work. After the morning lecture, which in the summer semester was at eight, he would come round the laboratory and see how every one was getting on, though of

course he spent more time with those who were doing joint work with him than with those who were working with the other professors. We used very often to see him again during the morning, and at least once in the afternoon. In 1891, when I first went to Heidelberg, the most important work being done was on the slow combustion of electrolytic gas, and, not only those who were actually working on this subject, but every one else in the laboratory, used to take great interest in what was going on. Meyer's way of taking up old pieces of his work again and again meant that a very varied sort of work was done in the laboratory; for example, in consequence of Nef's criticism, nitro-fatty compounds were again examined, the result on the students being excellent, in that their general interest in chemistry was aroused. Meyer, as you well know, was an excellent speaker, not only in the lecture room, but in taking the chair at meetings of the Heidelberger Chemische Gesellschaft, when he was seen to great advantage.

Dr. Sudborough, who served him as an assistant when in Heidelberg, writes :—

As organiser and director of the laboratories, Meyer undoubtedly exhibited great business qualities, and everything worked extremely smoothly, owing probably to the fact that his staff had been with him for a number of years and were all on intimate terms of friendship with him. During the time I was an assistant, I had opportunities of observing with what care he entered into even the minutest financial details in connection with the department.

In the laboratories he was extremely genial and pleasant, always having a kindly word for the students, and taking a great interest in their work. Characteristic, too, was the hopeful way in which he always looked forward to the successful termination of each piece of work, and by this means endeavoured to keep up the interest of the student. Every student who worked under him respected and honoured him as a scientific leader of the first rank, but in addition they felt a deep friendliness towards him on account of the kindly interest he took in them and in their work. A goodly percentage of those carrying out "Arbeits" under him were either English or American; in fact, Meyer appears to have had a predilection for English and American students.

Mention must also be made of Meyer's connection with the Heidelberg Chemical Society, of which he was an ardent supporter —in fact, may be said to have been its soul: he and Bernthsen (of the Badische Anilin- u. Soda-Fabrik) were the two presidents in my time and both were frequent contributors.

Dr. Jocelyn F. Thorpe writes:—

Perhaps that which impressed one most about Victor Meyer, besides his power and ability as an exponent and lecturer, was the faculty he possessed of conferring some of his enthusiasm upon the students who worked under him. Be his interest ever so slight and his knowledge of the subject ever so small, no student could work long with Victor Meyer without feeling that he had a part in a system; that he was, in fact, one of the instruments by which the plans and ideas of a master mind were being shaped. Noticeable, too, was his wonderful power of grasping and remembering every little detail of the researches upon which he was at the moment engaged. At times, as many as thirty men would be working under him on subjects widely varying in character, yet in no case would he forget what each individual had been doing when he last visited him; occasionally he would astonish the student by asking him what had become of some (by the student) long-forgotten substance, the properties of which he could remember most distinctly.

Every morning after his lecture he would enter the laboratories and personally not only visit the students who were working directly under him, but also those who were engaged in research with the other Professors and Privatdozenten of the department.

Not only will Victor Meyer live in the memory of those who worked under him as a leader in chemistry, but many will remember him as the genial and kindly host. On many evenings during the semester he would give either supper-parties or small dances, and occasionally a ball, to which his students were welcomed, and when his *camaraderie* and great tact were especially noticeable to those of us who, being foreigners newly arrived, were unacquainted with the language and social customs of his country.

Towards the last his wonderful memory began to fail. I remember meeting him in Heidelberg the year before his death and asking him something, I cannot now remember what, but at

any rate he was unable to answer me at the time, and asked me to call and see him the next day. He, however, stopped and tied a knot on his pocket-handkerchief, saying, with a sad smile, "my memory is not what it was."

Meyer's literary ability, combined with his power of lucid exposition, made him an admirable writer of what are called popular science articles. He was a frequent contributor to the *Naturwissenschaftlichen Rundschau*, and a number of his essays appeared under the title of *Aus Natur und Wissenschaft* (Heidelberg, 1892). His love of natural scenery and his power of graphically describing it may be seen in his *Märztage in Kanarischen Archipel* (Leipzig, 1893), a record of travel, written during the enforced rest following upon one of his too frequent periods of nervous prostration.

One who had studied him carefully and knew him well thus writes of his personal qualities and of the influence and attraction he exerted upon all who came in contact with him :—

Victor Meyer had a remarkable power over men. Where he entered, there he soon became the centre; each one listened to him, all collected round him. In the fascination he exercised there was nothing intentional or self-conscious; it was far less the influence of a commanding strength than the working of an incomparably attractive and many-sided nature. To this, too, his appearance contributed: the finely chiselled head with the splendid blue eyes might, at first sight, betoken the artist, were it not that there was in the expressive features a rare blending of the lively temperament with the contemplative calm of the philosopher.

In the circle of his fellows he captivated all by the eagerness with which he followed anything new, discussing and elucidating it in a manner peculiarly his own; by the joyous, often enthusiastic, recognition of other men's work, and by the warm-hearted interest he displayed in the scientific struggles of his juniors. In society, he showed himself an accomplished conversationalist and *raconteur*, an intelligent and warmly

appreciative connoisseur of the arts of music and literature. As a host, he studied the comfort of every guest in his house. At the "Biertisch" his sunny humour and overflowing wit diffused a general harmony; and his enthusiastic love of natural scenery made him the most delightful of travelling companions (Paul Jacobson, in *Naturwiss. Rundschau*, xii. 43 and 44, p. 19).

Meyer's merits were recognised in every land where science is cultivated. He was a corresponding member of the Academies of Munich, Berlin, Upsala, and Göttingen, and an honorary member of many learned societies. The University of Königsberg made him an Honorary Doctor of Medicine. The Royal Society gave him the Davy Medal in 1891.

He was elected an Honorary Foreign Member of our Society in 1883, and attended the celebration of our Jubilee in the spring of 1891. Many of our Fellows will no doubt recall the stirring speech, instinct with a true eloquence, which he made at the banquet in responding to the toast of "Our Foreign Members," with its striking peroration:—"Möge die Chemical Society neben allen ihren anderen schönen Aufgaben auch in Zukunft ihre völkerverbindenden Ziele in so erfolgreicher Weise anstreben wie bisher, möge Sie blühen und gedeihen als eine Pflegstätte der Wissenschaft, für ihr Vaterland vorerst, aber nicht minder für alle Völker, welche sich im friedlichen Wettbewerb wissenschaftlicher Arbeit verbündet wissen."

May this aspiration be fulfilled! May the Chemical Society, in the years to come, continue, as in the past, to recognise, with a full and generous appreciation, the work of those across the seas who engage with us in the friendly rivalry of scientific labour!

And may our action in thus recording the services in our own annals of the gifted man whose

prosperous labour fills
The lips of men with honest praise,

tend in some degree, however small, to that consummation for which he so earnestly and so eloquently pleaded!

XIV

DMITRI IVANOWITSH MENDELEEFF

"Scientific Worthies," No. XXVI.—*Nature*, 27th June 1889.

Dmitri Ivanowitsh Mendeleeff was born on 7th February (N.S.), 1834, at Tobolsk, in Siberia. He was the seventeenth and youngest child of Ivan Paolowitsh Mendeleeff, Director of the Gymnasium at that place. Soon after the birth of Dmitri his father became blind, and was obliged to resign his position, and the family became practically dependent upon the mother, Maria Dmitrievna Mendeleeva—a woman of great energy and remarkable force of character. She established a glass-works at Tobolsk, the management of which for many years devolved entirely upon her, and on the profits of which she brought up and educated her large family. The story of Mendeleeff's youth is given in the preface to his great work *On Solutions*, which he dedicated to the memory of his mother in a passage of singular beauty and power. Having passed through the Gymnasium at Tobolsk, Mendeleeff, at the age of sixteen, was sent to St. Petersburg, with the intention that he should study chemistry at the University, under Zinin. He was, however, transferred to the Pedagogical Institute, the aim of which was to train teachers for the District or Governmental Gymnasiums throughout the Empire. The Institute

(which was abolished in 1858) was established in the same building as the University, and was divided into two Faculties — Historico-philological and Physico-mathematical. Mendeleeff attached himself to the natural sciences, and thus came under the influence of Woskresenky in chemistry, of Emil Lenz in physics, of Ostrogradsky in mathematics, of Ruprecht in botany, of F. Brandt in zoology, of Kutorga in mineralogy, and of Sawitsh in astronomy, most of whom were professors of the same sciences in the University. Whilst at the Institute he wrote his first paper on "Isomorphism," and on the termination of his course of instruction he was appointed to the Gymnasium at Simferopol, in the Crimea. During the Crimean war he was transferred to one of the Gymnasiums at Odessa, and in 1856 he was admitted to the degree of *Magister Chemiæ* of the Physico-mathematical Faculty of the University of St. Petersburg, and was made *Privat-Docent* in the University. Even at this early period of his career we find Mendeleeff speculating on the great problems with which his name is inseparably connected. The relations between the specific gravities of substances and their molecular weights had begun to attract increased attention. Kopp had just published the first instalment of that long and laborious series of experimental observations which constitutes the real foundation of all our knowledge concerning the specific volumes of liquids, when the young Siberian philosopher laid a number of theses on problems relating to specific volumes before the Physico-mathematical Faculty of the University. He pointed out that magnetic elements have smaller specific volumes than diamagnetic elements. He also showed that Avogadro's supposition, that electro-positive elements have larger

specific volumes than electro-negative elements, was in accordance with the greater number of well-established facts. When we remember how slowly, in spite of the powerful advocacy of Williamson, the ideas of Laurent and Gerhardt, and what came to be known as the modern French school, found favour in this country, it is remarkable, as indicating the radical and progressive character of his mind, and the keenness of his mental vision, to find Mendeleeff, as far back as 1856, insisting that to Gerhardt was due the best mode of determining the chemical molecule; that the molecule of oxygen was expressed by the symbol O_2; those of arsenic and phosphorus by As_4 and P_4 respectively; that of alcohol by $\left.\begin{matrix}C_2H_5\\H\end{matrix}\right\}O$; and that of ether by $\left.\begin{matrix}C_2H_5\\C_2H_5\end{matrix}\right\}O$.

Mendeleeff's researches on specific volumes were begun in 1855, and were continued, with intermissions, down to 1870; but part only of the work has been published. In 1859 he obtained permission from the Minister of Public Instruction to travel, and repaired to Heidelberg, where he established a small private laboratory, and occupied himself with the determination of the physical constants of chemical compounds. He returned home in 1861, and in 1863 was named Professor of Chemistry at the Technological Institute of St. Petersburg. In 1866 he became Professor of Chemistry at the University, and was made Doctor of Chemistry after a public defence of his dissertation "On the Combinations of Water with Alcohol." He is now Emeritus Professor, and until recently delivered lectures on general chemistry.

Mendeleeff is so prolific a writer that it is impossible within the limits of an article of this kind to do justice

to his work. There is, in fact, no section of chemical science which he has not enriched by his contributions. Some of his earliest work related to questions of mineralogy and chemical geology; and at times, as in his papers on Œnanthol-Sulphurous Acid, on Fermentation Propyl Alcohol, and on the Nitriles, he cultivated the rapidly extending domain of organic chemistry. But his reputation mainly rests upon his contributions to physical chemistry and to chemical philosophy. In his papers on Specific Volumes he extends Kopp's generalisations, and traces the specific volumes of substances through various phases of chemical change. He shows that in the thermal expansion of homologous liquids the expansion-coefficient diminishes in a regular manner as the series is ascended, and he indicates the intimate connection which exists between expansion and cohesion in the case of liquids, and the *rôle* played by molecular cohesion in the determination of chemical activity. His paper on the thermal expansion of liquids above their boiling points is noteworthy as demonstrating that the empirical expressions given by Kopp, Pierre, and others, for the expansion of liquids up to their boiling points, are equally applicable to far higher temperatures, and that the expansion-coefficient gradually increases with the diminution in molecular cohesion of the liquid, until, as in ether at 133°, it becomes even greater than that of the gas. The expansion-coefficient of ether increases to 0·0054 at the temperature of its *absolute boiling point*—that is, at about 190°. The absolute boiling point is defined by Mendeleeff as that temperature at which the cohesion and latent heat of vaporisation are *nil*, and at which the liquid becomes gaseous independently of pressure and volume. It is, in fact, that temperature which the researches of

Andrews have made us familiar with as the "critical-point." In this paper Mendeleeff presents us for the first time with a number of determinations of the critical-temperatures of various substances, founded partly on his own determinations, and partly on those of Cagniard de la Tour, Wolff, and Drion.

Other papers on physical chemistry relate to Contact Action, to Fractional Distillation, and to the Heat of Combustion of Organic Substances. In 1883 Mendeleeff was made an honorary member of our Chemical Society, and in the following year he contributed a remarkable paper to the Journal of the Society (*Transactions of the Chemical Society*, xlv. 126), in which he developed an extremely simple general expression for the expansion of liquids under constant pressure between 0° and their boiling points. This expression may be written $1/V_t = 1 - kt$, in which V_t is the volume at $t°$ (that at 0° being unity), and k is a quantity which varies with different substances, but which may for any one substance be considered invariable between 0° C. and the neighbourhood of the boiling point. This formula is analogous to that which expresses Dalton's law of the uniformity of expansion of gases. But just as the formula, $V = 1 + kt$, applies only to a so-called ideal gas, Mendeleeff's expression is in like manner to be regarded only as a first approximation—that is, as applicable only to ideal liquids. The deviations are not large in either case; they are, as might be expected, largest near temperatures at which the states of the bodies change. In the case of actual liquids the deviations from the ideal form of expansion increase not only as the liquid approaches the point at which its state of aggregation is changed, but also augment with diminishing density, increasing cohesion, and diminish-

ing molecular weight. This last cause is especially noteworthy since Mendeleeff showed, more than a dozen years ago (*vide supra*), that the deviations from Dalton's law were related to the molecular weights of the gases. The well-known irregularities in the expansion of water are, according to Mendeleeff, connected with its small molecular weight, its high surface-energy, and the comparatively small temperature-interval within which its state of aggregation is unchanged. Subsequent observers, by applying Van der Waal's theory of the general relation between the pressure, volume, and temperature of bodies to Mendeleeff's expression for the thermal expansion of an ideal liquid, have shown that the reciprocal of the constant k is the number obtained by subtracting 273 from the product of the critical temperature into a quantity which should be the same for all substances. The value of this quantity is approximately 2, and since the range of its variation is apparently very small, at all events for certain classes of substances, the development of Mendeleeff's formula affords a simple and ready method of calculating the critical temperature of bodies from observations of their expansions as liquids.

Mendeleeff's skill in physical measurement is well illustrated by his determinations of the Specific Gravities of Aqueous Solutions of Alcohol. Such determinations have been frequently made the subject of rigorous experiment in this and other countries, inasmuch as they constitute the basis of the methods of assessing the duty on spirits, which is so important a factor in the national income of many States. Mendeleeff's work has served to confirm and extend that of Drinkwater, Fownes, and Squibb, and has been utilised by certain continental Governments (*e.g.* that of Holland)

for the purposes of revenue. But it was not the utilitarian aspect of this subject which alone attracted Mendeleeff. In a paper communicated a couple of years ago to our Chemical Society (*Transactions of the Chemical Society*, li. 778), these determinations are applied towards the elucidation of a theory of solution in which it is sought to reconcile Dalton's doctrine of the atomic constitution of matter with modern views respecting dissociation and the dynamical equilibrium of molecules. According to Mendeleeff, solutions are to be regarded as containing strictly definite atomic chemical combinations at temperatures higher than their dissociation temperature, and just as definite chemical substances may be either formed or decomposed at temperatures which are higher than those at which dissociation commences, so we may have the same phenomenon in solutions; at ordinary temperatures they can be either formed or decomposed. In addition, the equilibrium between the quantity of the definite compound and of its products of dissociation is defined by the laws of chemical equilibrium, which require a relation between the masses of the active components present in equal volumes (*loc. cit.* p. 779). It follows from this hypothesis that the specific gravities of solutions depend on the extent to which active substances are produced, or that the expression for the specific gravity, s, as a function of the percentage composition, p, may be represented by the general equation—

$$s = C + Ap + Bp^2.$$

Between the densities of two definite compounds existing in solution, the differential coefficient ds/dp is a linear function of p—

$$ds/dp = A + 2Bp.$$

By the application of this method to the case of aqueous solutions of ethyl alcohol, Mendeleeff infers the existence of three definite hydrates, viz. $EtHO.12H_2O$; $EtHO.3H_2O$; and $3EtHO.H_2O$, the first two of which he has isolated by subjecting the mixture to low temperatures. The hypothesis respecting the linear character of the differential coefficient ds/dp has been proved to be correct for solutions of many salts, of acids, and of ammonia.

We have the consummation of this work on solution in the monograph published by Mendeleeff in 1889. This volume, the fruit of many years of labour, is unquestionably one of the most important contributions to the theory of solution yet given to science.

Much of Mendeleeff's scientific activity since 1871 has been absorbed in an extended work on the elasticity of the gases, which he has executed in conjunction with his pupils, Kirpitshoff, Hemilian, Bogusky, and Kajander. Portions only of the results have as yet appeared. The first volume, published in Russian in 1875, contains details of the modes of measurement; these involved many forms of apparatus new to physical science. A summary of the principal results obtained was published in the form of a pamphlet in 1881. Regnault found that the product $p.v = \text{const.}$—*i.e.* Boyle's law—was true for ideal gases only. Between one atmosphere and thirty atmospheres the deviations were positive in the case of hydrogen, and negative in those of all other gases. Mendeleeff pointed out that the deviations must become positive for all gases at sufficiently high pressures, and the fact has since been confirmed by the observations of Amagat and Cailletet. Mendeleeff, more particularly, made observations at low pressures, *i.e.* below one atmosphere; and

here the deviations were again found to be positive and relatively very large. It was found, in fact, that, at the limit of condensation, the gases seemed to behave like solid bodies—*i.e.* the molecules were incapable of being separated or brought nearer together to any appreciable extent by varying pressure. Mendeleeff has further determined the *real* coefficients of thermal expansion of gases. This, for air between 0° and 100° under a standard atmosphere, was found to be 0·0036829. Determinations made in the case of other gases have shown that the coefficients of expansion increase with increasing molecular weight, gases of the same molecular weight giving the same coefficient.

	Molecular weight.	Coefficient of expansion.
Hydrogen	2	0·00367
Nitrogen Carbon monoxide	28	0·00373
Nitrous oxide Carbon dioxide	44	0·00373
Sulphur dioxide	64	0·00385
Hydrogen bromide	81	0·00386

The coefficient of expansion is found to decrease with increasing pressure in the case of hydrogen. Thus at

200 mm.	0·00369
760 „	0·00367
8 atmos.	0·00366

But with the so-called coercible gases the reverse is found to take place. Thus, in the case of carbon dioxide,

120 mm. pressure	0·00372
220 „ „	0·00370
760 „ „	0·00373
3 atmos. „	0·00389
8 „ „	0·00413

The decrease of the coefficient of expansion with

increasing pressure is a normal phenomenon of gases, the positive deviation observed in the case of hydrogen being found to hold equally good for all gases at very high and very low pressures. Hence the laws of Boyle and Dalton are only valid at points of the curve where the deviation changes from positive to negative, or *vice versâ*.

These experiments have also borne fruit in various meteorological papers on the physical nature of the highly rarefied air existing in the upper strata of the atmosphere. In this connection it may be stated that Mendeleeff has attempted to organise meteorological observations in the upper regions of the atmosphere by means of balloons, and hence he has been led to study aëronautics. His practical acquaintance with the subject induced him to make an ascent from Klin during the total solar eclipse of 19th August 1887, for the purpose of observing the extension and structure of the corona when seen through highly rarefied air.

Russia is indebted to Mendeleeff for the training of two generations of her chemists. His writings have largely modified the mode of teaching chemical science in that country. His treatise on Organic Chemistry was the standard work of its time, and exercised great influence in spreading abroad the conceptions which are associated with the development of modern chemistry. His *Principles of Chemistry*, published in 1869, and repeatedly reprinted, is a veritable treasure-house of ideas, from which investigators have constantly borrowed suggestions of new lines of research. This book is one of the classics of chemistry; its place in the history of science is as well assured as the ever-memorable work of Dalton. Mendeleeff, indeed, might with equal fitness have styled his book a "New System of

Chemical Philosophy." In it he has developed the great generalisation which is known under the name of the "Periodic Law"—a generalisation which is exerting a profound influence on the development of chemical science in all countries in which its study is actively prosecuted. Mendeleeff first drew attention to the principles upon which the Periodic Law is based in a paper read to the Russian Chemical Society in 1869, in the following series of propositions :—

(1) The elements, if arranged according to their atomic weights, exhibit an evident *periodicity* of properties.

(2) Elements which are similar as regards their chemical properties have atomic weights which are either of nearly the same value (*e.g.* platinum, iridium, osmium), or which increase regularly (*e.g.* potassium, rubidium, cæsium).

(3) The arrangement of the elements, or of groups of elements, in the order of their atomic weights, corresponds with their so-called *valencies*, as well as, to some extent, with their distinctive chemical properties; as is apparent, among other series, in that of lithium, beryllium, boron, carbon, nitrogen, oxygen, and fluorine.

(4) The elements which are the most widely diffused have *small* atomic weights.

(5) The *magnitude* of the atomic weight determines the character of the element, just as the magnitude of the molecular weight determines the character of a compound body.

(6) We must expect the discovery of many yet *unknown* elements—for example, elements analogous to aluminium and silicon—whose atomic weight would be between 65 and 75.

(7) The atomic weight of an element may sometimes

be amended by a knowledge of those of the contiguous elements. Thus the atomic weight of tellurium must lie between 123 and 126, and cannot be 128.

(8) Certain characteristic properties of elements can be foretold from their atomic weights.

In his memorable Faraday Lecture delivered to the Chemical Society, and from which these words are taken, Mendeleeff has indicated the lines upon which the evolution of his theory proceeded. In the first place, it is to be noted that it is based wholly on experiment; it is as much the embodiment of fact as are the laws of chemical combination formulated by Dalton. Without the knowledge of certain data it could not possibly have been discovered; with this knowledge its appearance, says Mendeleeff, is natural and intelligible. Three series of data were necessary to pave the way for its enunciation :—

(1) The adoption of the definite numerical values of the atomic weights founded on the conceptions of Avogadro and Gerhardt, as insisted upon by Cannizzaro. (2) The recognition that the relations between the atomic weights of analogous elements were governed by some general law. Many chemists, and more especially Dumas, Gladstone, and Strecker, had drawn attention to the numerical relationship existing between correlated groups of elements, but no one before Newlands in England, and De Chancourtois in France, had sought to generalise this conception, and to extend it to all the elements by considering their properties as functions of their atomic weights. (3) A more accurate knowledge of the relations and analogies of the rarer elements, such, for example, as that given to us by Roscoe in the case of vanadium, and by Marignac in that of niobium. The law of periodicity was the systematised expression

of these data; it was, to use Mendeleeff's language, "the direct outcome of the stock of generalisations of established facts which had accumulated by the end of the decade 1860-70."

We can only very rapidly allude to some of the more striking services which Mendeleeff's generalisation has rendered to science during the twenty years of its existence. By a more systematic arrangement and co-ordination of the known chemical elements, it has not only indicated the existence of new forms of elementary matter, but it has pointed out the probable sources of the undiscovered substances, and has enabled us to know their properties even before we have knowledge of their existence. It was this power of divination inherent in the law which, perhaps more than any other feature, first attracted attention to it, and quickened the interest with which its development was regarded by men of science. There are now three instances of elements of which the existence and properties were foretold by the periodic law:—(1) That of *gallium*, discovered by Boisbaudran, which was found to correspond with the *eka-aluminium* of Mendeleeff; (2) That of *scandium*, corresponding to *eka-boron*, discovered by Nilson; and (3) That of *germanium*, which turns out to be *eka-silicium*, by Winckler. No one who was present on the occasion of the delivery of the Faraday Lecture will forget the enthusiasm which followed the reading of these words of Mendeleeff's: "When, in 1871, I described to the Russian Chemical Society the properties, clearly defined by the periodic law, with such elements ought to possess, I never hoped that I should live to mention their discovery to the Chemical Society of Great Britain as a confirmation of the exactitude and the generality of the periodic law."

Up to the time of the formulation of the law, the determination of the atomic value or valency of an element was a purely empirical matter, with no apparent necessary relation to the atomic value of other elements. But to-day this value is as much a matter of *a priori* knowledge as is the very existence of the element or any one of its properties. Striking examples of the aid which the law affords in determining the substituting value of an element are presented by the cases of *indium*, *cerium*, *yttrium*, *beryllium*, *scandium*, and *thorium*. In certain of these cases, the particular value demanded by the law, and the change in representation of the molecular composition of the compounds of these elements, have been confirmed by all those experimental criteria on which chemists are accustomed to depend. One of the most interesting instances of the kind is afforded by *uranium*, the atomic weight of which was formerly regarded as 120, then as 180, but which, on the authority of the periodic law, is now established as 240, a value confirmed by the independent experiments of Zimmermann and Rammelsberg. Uranium has a special interest in being the last term in the series; no element of higher atomic weight is at present known.

As examples of the value of the law in enabling us to correct the atomic weights of elements whose valencies and true position were well known, we may cite the cases of *gold*, *tellurium*, and *titanium*, the values of which were apparently higher than those demanded by it. In each of these cases a redetermination of the atomic weight has resulted in a value which is in conformity with the previsions of the periodic law.

The law has, moreover, enabled many of the physical properties of the elements to be referred to the same

principle of periodicity. At the Moscow Congress of Russian physicists in August 1869, Mendeleeff pointed out the relations which existed between the density and the atomic weights of the elements; these were subsequently more fully examined by Lothar Meyer, and are embodied in the well-known curve in his *Modern Theories of Chemistry*. Similar relations have been discovered in certain other properties, such as ductility, fusibility, hardness, volatility, crystalline form and thermal expansion; in the refraction equivalents of the elements, and in their conductivities for heat and electricity; in their magnetic properties and electro-chemical behaviour; in the heats of formation of their haloid compounds; and even in such properties as their elasticity, breaking stress, etc.

In the Faraday Lecture, Mendeleeff indicated the bearing of the law of periodicity upon the doctrine of constant valency, and especially on the conception of a primordial matter. The mind almost instinctively clings to the notion that the law can only find its rational interpretation in the idea of unity in the formative material, and it is not surprising that the promulgation of the law has been heralded by some as the most convincing proof of the validity of the Pythagorean conception that experiment has yet been able to adduce. But the author of the periodic law will not admit that his generalisation has either sprung from this conception or has any relations towards it. "The periodic law, based as it is on the solid and wholesome ground of experimental research, has been evolved independently of any conception as to the nature of the elements; it does not in the least originate in the idea of an unique matter; and it has no historical connection with that relic of the torments

of classical thought; and therefore it affords no more indication of the unity of matter or of the compound nature of the elements than do the laws of Avogadro and Gerhardt, or the law of specific heats, or even the conclusions of spectrum analysis. None of the advocates of an unique matter has ever tried to explain the law from the standpoint of ideas taken from a remote antiquity, when it was found convenient to admit the existence of many gods—and of an unique matter."

No record of Mendeleeff's intellectual activity would be complete without some reference to his influence on the development of the industrial resources of Russia. In 1863 he brought out the first encyclopædia of chemical technology of any magnitude which the literature of that country possessed, and he has been frequently commissioned to report on the progress of chemical industry as manifested at the various International Exhibitions. But it was on the petroleum industry of Baku, on the Caspian, that this influence has been most widely felt. Fifteen years ago the production of petroleum in Russia was a monopoly, and was accompanied by all the evils which usually spring from monopolies; the trade was exceedingly limited, and apparently incapable of development. Thanks largely to his action, both on the platform and in the press, the exploitation of the boundless supplies of the peninsula of Apsheron was thrown open to the world, with the result that petroleum threatens to effect an industrial revolution in Eastern Europe and in Asia. Indeed, it is not too much to say that the oil industry of Baku is rapidly becoming, directly and indirectly, one of the most powerful factors in the Central Asian problem. Mendeleeff's interest in the development of the Baku industry has led to his being sent to the

Caucasus and to Pennsylvania, to report upon the best modes of working the wells, and of separating and utilising the products. Last year, during the coal crisis in Southern Russia, he was commissioned to study the economic condition of the industry in the rich coal-basin of Donetz.

No man in Russia has exercised a greater or more lasting influence on the development of physical science than Mendeleeff. His mode of work and of thought is so absolutely his own, the manner of his teaching and lecturing is so entirely original, and the success of the great generalisation with which his name and fame are bound up is so strikingly complete, that to the outer world of Europe and America he is to Russia what Berzelius was to Sweden, or Liebig to Germany, or Dumas to France. Nowhere has Mendeleeff's pre-eminence been more quickly or more fully recognised than in this country. In 1882 the Royal Society gave him the Davy Medal; and in 1889 the Chemical Society, which is proud to number him among its Honorary Fellows, conferred upon him the highest distinction in its power by the award of the Faraday Medal. To the great regret of the large gathering of British chemists which had assembled to welcome him and to listen to the remarkable address on the subject which he of all others is best fitted to expound, Mendeleeff was unable to receive the gift in person; but the circumstances of his absence awakened a deep feeling of commiseration and sympathy, and served to intensify the sentiment of respect and admiration with which he is regarded by all British men of science.[1]

[1] I have to express my grateful acknowledgments to Professor Menshutkin and M. Gorboff, of St. Petersburg, and to Dr. B. Brauner, of the University of Prague, for much of the information on which this article is based.

XV

STANISLAO CANNIZZARO

"Scientific Worthies," No. XXX.—*Nature*, 6*th May* 1897.

In the autumn of last year (1896) there occurred in Rome an event which attracted the attention of the whole scientific world, and more especially of that portion of it which is concerned with chemistry. The occasion was the celebration of the seventieth anniversary of the birth of Prof. Stanislao Cannizzaro, Senator of the Kingdom of Italy, and Professor of Chemistry in the University of Rome. The pages of this journal have already borne witness to the feelings of esteem and gratitude which that event evoked. At the public meeting called to do him honour, all the learned bodies in the world which have any concern with science, or have any regard for its welfare, combined to offer their felicitations, and vied with each other in the warmth of their expressions of appreciation and goodwill, and a multitude of letters and telegrams were received from chemists in all parts of Europe and America. The place of honour in the list of the addresses, as enumerated in the interesting account of the ceremony since published, is given to that from the Royal Society, which repeated the terms in which the Council had previously made known to Prof. Cannizzaro its reason for awarding him

the highest distinction in its power. Next comes that from the Chemical Society, which recalls with pride that the name of Cannizzaro has given lustre to the roll of its foreign members for more than half the period of his lifetime.

In what follows we desire to give an account of the life and labours of one whom men of all nations have thus shown themselves eager to honour.

Stanislao Cannizzaro, the fourth and youngest son of Mariano Cannizzaro and Anna Dibenedetto, was born on 13th July 1826 at Palermo, where his father was a magistrate, Director-General of the Sicilian Police, and subsequently President of the High Court of Chancery. The future chemist was educated partly at home and partly at the normal school of his native city, and on the death of his father in 1836, he was placed in the Carolino Calasanzio College. The cholera epidemic of 1837 ravaged Palermo, the young Cannizzaro lost two of his brothers, he himself was attacked by the terrible scourge, and it was only after a tedious convalescence that he was able to resume his studies. Elementary education in Sicily at that time was wholly under the control and direction of the priests: grammar, rhetoric, poetry, and philosophy, with a very small modicum of mathematics and geography, constituted the pabulum on which the youth of the period was fed. The physical sciences, of course, had no place in a system which was essentially mediæval. The boy soon gave evidence of his power, and after a school career of distinction he entered, in 1841, the University of Palermo with the intention of devoting himself to medicine.

The subject, however, proved uncongenial, and the youth tried in vain to pass the necessary examinations.

Stimulated, however, by Foderà, who at that time taught physiology in Palermo, and with whom the young student became intimately acquainted, he was led to take up experimental work in connection with chemical physiology. It is needless to say that at this period Palermo possessed no laboratory accommodation, and all the manipulative essays that the young experimentalist could venture upon had to be done at his home, and with such improvised appliances as he could command. In the autumn of 1845 he went to Naples, where he came in contact with Melloni, the most eminent Italian physicist of his time, with whom he contracted a warm friendship. Mainly through the recommendation of Melloni, who quickly learned to appreciate the character and power of his young friend, Piria, who is honourably known to chemists by his researches on plant products, was led to offer the young Sicilian the post of *preparateur* in the chemical laboratory of the University of Pisa. To Pisa accordingly he went, and the step decided his career. What Melloni was to physics in Italy at that period, Piria was to chemistry. The young assistant could have had no better master. Raffaele Piria, then in the full tide of his vigour, was an admirable, albeit a most exacting teacher. A distinguished pupil of Dumas, and a remarkable expositor, his lectures were distinguished by the same love of method, of orderly arrangement, of precision, neatness, and even elegance, that characterised his laboratory work; and Cannizzaro and his fellow-assistant Bertagnini must at times have been sorely exercised to satisfy the rigorous ideal of exactitude and of manipulative skill required by the Professor in the experimental illustration of his lectures. When not employed in the class-room, his duty was

to wait on Piria in the laboratory. Piria during that period was engaged upon those inquiries on salicin, populin, asparagin, and their derivatives, by which he is best known to the chemists of this time. The greater part of the experimental labour connected with these investigations was done by Piria himself during the eight hours that he daily spent in his laboratory, Cannizzaro being for the most part, as he says, a simple looker-on, observing attentively and in silence the rare skill and manipulative ability with which the work was executed. Occasionally, however, the assistant would be called upon to continue some experiment or analysis which Piria had begun, or to prepare some material he needed; all of which he was required to perform in literal compliance with the instructions he received from the master. Most of the work of preparation in connection with the lectures had to be done in the early morning, before Piria descended from his apartments to the laboratory. These preparations were carefully scrutinised by the Professor, who would tolerate no slovenliness or negligence, and whose æsthetic sense demanded that the apparatus should not only work well but look well. Although a silent worker during the daytime, and a most severe judge of his assistant's duty whether in the laboratory or in the lecture-room, Piria could unbend in his hours of ease, and many an evening was spent by Cannizzaro with his master, who would then freely discuss chemical subjects with his young assistant, and explain the object and meaning of the work on which he had been engaged during the day.

This severe discipline, to which Cannizzaro frankly confesses he owes much of the success of his after career as a chemist, was interrupted by events which, as they

have turned out, had no small share in determining also his success in his career as a politician. Returning to Sicily at the end of July 1847, presumably to spend his vacation at home, the ardent young Liberal of twenty-one, mindful of the events of 1836, naturally found himself in active sympathy with the movement of the time, and when the revolution broke out in January 1848, he became an officer of artillery at Messina. Having been elected deputy for Francavilla in the Sicilian Parliament, he went to Palermo at the end of March, and, as the youngest member of the Assembly, he was required to act as its Secretary. After the bombardment and fall of Messina on 7th September 1848, he was sent to Taormina to organise resistance to the advance of the royal troops. The armistice of 13th September, extorted by the combined fleets of England and France to put a stop to the atrocities of Ferdinand's army, stayed for the moment further hostilities, but Cannizzaro was ordered to remain at his post as Commissioner of the provisional Government. The armistice ended in the following March, and after the disaster of Novara, and with it the abdication of Charles Albert, the Sicilian movement utterly collapsed. The royal troops were everywhere victorious, the insurgents retreated first to Catania and thence by Castrogiovanni to Palermo, and, in May 1849, Cannizzaro, with a number of his compatriots, succeeded in escaping for Marseilles on board the Sicilian frigate *Indipendente*. He was now in exile, and led for a while a somewhat wandering and aimless existence. After a short stay in Marseilles, he passed on to Arles, and visited in turn Avignon, Lyons, Nîmes, and Montpellier. In time, however, he again betook himself to his chemical studies, although his means were very limited and his oppor-

tunities few. He had, of course, no laboratory, but he read such books as he could obtain, and visited such chemical factories as would admit him. When the body of the unfortunate and broken-hearted Charles Albert was brought back from Oporto, to be buried in the land for whose liberty he had sacrificed his kingship, Cannizzaro joined his fellow-refugees in Turin in order that they might testify by their presence at the obsequies of the dead monarch their grateful memory of his services, and their resolution that his tomb on the Superga should be to them the symbol of an undying aspiration.

Towards the end of October, Cannizzaro found himself in Paris. Thanks to a letter from Piria, he became acquainted with Cahours, who introduced him into the little laboratory of Chevreul attached to the theatre in the Jardin des Plantes, where he found Cloëz installed as assistant. He had now abundant opportunities for work, and with the characteristic ardour of his Southern blood he embraced them all. The excitement of political disquietude in Paris has never seemed to react disastrously on the progress of science there. Curiously enough, for some inscrutable reason, it would appear to stimulate it. Indeed, some of the darkest and most unsettled periods of the political history of France have been among the brightest and most glorious epochs in the annals of science. The stir of 1848, and the unrest which followed it, were contemporaneous with an extraordinary activity in chemical and physical inquiry in Paris, and Cannizzaro participated to the full in the busy movement going on around him. Dumas, it is true, had been swept by his political convictions into the Legislative Assembly, to become Minister of Agriculture and Commerce; and

his laboratory in the Rue Cuvier, in spite of the seductive offer of Jecker, was closed.

Still, if Cannizzaro never came under the spell of Dumas, he could witness Fremy's experiments in the laboratory of Gay Lussac, and could attend Regnault's lectures in the Collège de France. But it was to the chemical work-table he mainly turned, and on this he spent the greater part of his time and energies. He took up the study of the amines, the existence of which had recently been made known by Wurtz, and, with Cloëz, prepared cyanamide by the action of ammonia on cyanogen chloride. An account of the nature and properties of this compound, published in 1851 in conjunction with Cloëz, constitutes Cannizzaro's first contribution to the literature of chemistry. The reaction by which they obtained the substance proved exceedingly fruitful, and, by the substitution of amines for ammonia, Cahours and Cloëz subsequently prepared the alkyl cyanamides. Moreover, cyanamide itself, by the ease with which it suffers polymerisation, gives rise to a number of isomeric series of homologous amides of considerable theoretic interest. Congenial and inspiriting as the atmosphere of Paris might be, man cannot live on air alone. But there were too many young and eager aspirants, of French nationality, for the few posts which practically only Paris was able to offer, to justify the hope that the young Sicilian could obtain a position, sufficiently lucrative even for his modest requirements, in the land of his exile. Piedmont, of all the Italian States, could alone afford an asylum to him, and accordingly, towards the end of 1851, he accepted the position of Professor of Physical Chemistry and Mechanics in the National College of Alessandria, an institution modelled some-

what on the lines of a German Realschule. Here, thanks to the action of the municipality, he was provided with a small laboratory, together with an assistant, and, although much occupied by his public lectures on chemistry and mechanics given to the townspeople, in addition to his regular class instruction, he began the study of the action of alkylamines on cyanogen chloride, only to find himself forestalled by Cloëz and Cahours. At about the same time he discovered benzyl alcohol, which he obtained by the action of alcoholic potash on bitter almond oil, and the properties and modes of decomposition of which he described in a series of letters to Liebig and Wöhler, published in the *Annalen*. His vacations were usually spent with Piria at Pisa, or at Montignoso, near Massa-Carrara, with his old collaborateur Bertagnini, with whom he worked on anisic alcohol (*Ann. de Chimie*, xlvii. 285).

In October 1855 he was called to the chair of Chemistry at the University of Genoa, and at the same time Piria was moved from Pisa to Turin. Although the new position at Genoa was one of greater dignity and emolument, Cannizzaro found himself, so far as laboratory accommodation was concerned, less favourably situated than at Alessandria; the only place at his disposal was a damp and dimly-lighted room, without the slightest convenience for even the most elementary experiments. For some months he found it impossible to carry on the work he had begun at Alessandria. In the following year he obtained a room on the upper floor of the University building, and this, with the aid of an assistant and a couple of pupils, he turned into a fairly convenient laboratory, where he resumed his work on the aromatic alcohols. At Genoa Cannizzaro

began the studies on chemical philosophy which were to culminate in the great generalisation with which his name will continue to be associated. Admirable as his experimental labours are, his chief claim to the esteem and gratitude of his contemporaries and of posterity rests upon his critical contributions to the philosophy of chemistry. In what this signal service consisted will be shown subsequently.

During the whole of this time Italy was in a state of political ferment. The astute Cavour had gradually secured his ascendency in the parliamentary councils of the little Sardinian kingdom, and with it his position in the councils of Europe. Slowly, and in spite of many checks, the cause of Italian unity gained ground. Magenta and Solferino secured Lombardy, and although Victor Emmanuel was forced to give up Savoy, the very cradle of his dynasty, as the price of Louis Napoleon's co-operation, Italy gained Tuscany, Modena, Parma, and Romagna; and in 1860 the annexation of Central Italy was complete. Bombino still held his grip on the two Sicilies, but the islanders made one more effort to throw off the hateful yoke. The time seemed propitious, and Palermo, Messina, and Catania were soon ablaze; and before the middle of May Garibaldi and his famous "Mille" had accomplished the liberation of the island. Cannizzaro immediately returned to Palermo. He found here his aged mother and sisters, whom he had not seen since 1849, and at once threw himself into the labour of organising and consolidating the work of the revolution, taking an active part in the debates of the States Council convened to define the relation of Sicily to Italian unity. The affair of Spartivento to all intents and purposes decided the fate of Lower Italy, and by the first week of September Garibaldi was in Naples, and with the

shutting up of the last and feeblest of the Neapolitan Bourbons in Gaeta, the emancipation of Italy was practically secured. What remained to be done time would effect.

Cannizzaro now returned to Genoa, passing through Naples, where Piria had been called to reorganise the system of public instruction, and resumed his work at the University. In the preceding March he had been offered, but had declined, the Professorship of Organic Chemistry in the University of Pisa. He was now invited to occupy the chair on the same subject in the University of Naples, and this he also refused. He was then claimed by his native town, and in October 1861 he was named Professor of Inorganic and Organic Chemistry, and Director of the Laboratory of the University of Palermo. What he had to "direct" was contained in a few cupboards, in the same classroom that he had sat in as a student in 1842, and was barely sufficient for even the most elementary illustrations. The whole of the following year was spent in organising his courses and in superintending the arrangement and plenishing of the rooms he ultimately acquired on the top floor of the University building.

Cannizzaro remained at Palermo for about ten years; he took an active share in the management of the University, and for a time was its Rector. Its influence as a school of chemistry may be judged of from the fact that he had as co-workers Adolph Lieben, Wilhelm Koerner, and lastly Paterno, who has succeeded him in the chair. For the most part he occupied himself, as regards his laboratory work, with the study of aromatic compounds, and in extending and completing his researches on the amines.

If Cannizzaro was useful to the world as a chemist,

he was so far mindful of Priestley's example as to strive to be equally useful to Palermo as a citizen, and much of his time and ability was freely given in the service of her municipal government, more particularly on subjects relating to elementary and secondary education.

In 1871 Cannizzaro was called to occupy his present position of Professor of Chemistry in the University of Rome, and Director of the Chemical Institute in the Orto di S. Lorenzo in Panisperma, and here, for the last five-and-twenty years, he has annually delivered his two courses, each of three lectures a week, on general and organic chemistry, and has worked out, partly alone and partly in conjunction with his pupils Amato, Blaserna, Carnelutti, Sestini, Valente, Fabris, and Andreocci, the chemistry of santonin. At the same time that he was called to Rome he was made a Senator of the kingdom, and as a moderate Liberal he has taken his share in the consolidation of the constitution of regenerated Italy.

Cannizzaro, when compared with such men as Berthelot and certain of the leaders of the German schools of chemistry, or even with some of the younger generation of Italian chemists, cannot be called a voluminous writer. In all about eighty memoirs have proceeded from his laboratory. It is on the special quality and character of his published work, rather than on its extent, or on the range and variety of its subject-matter, that his fame depends. In this respect he resembles the late August Kekulé. The names of both men will for ever be associated in the history of chemistry with the promulgation of generalisations which mark epochs in the development of chemical science. Cannizzaro's great merit consisted in being the first to clearly point out the bearing on chemical theory of the hypothesis which is commonly associated

with the name of his countryman Avogadro, but which Cannizzaro himself, in his well-known lecture delivered before the Fellows of the Chemical Society in 1872, associated also with the names of Ampère, Krönig, and Clausius. This, perhaps, is not the time and the place to discuss the question of whatever claims John Dalton may have to be the first to recognise the fundamental truth embodied in the statement that gases, under comparable conditions, contain in equal volumes equal numbers of molecules, whatever may be their nature and their weight. For the moment we are concerned only with the fact that it remained to Cannizzaro to show that the hypothesis afforded the means of placing the most important of all chemical constants—the atomic weights of the elements—on a definable and intelligible basis, and thereby of rendering our conceptions of atoms and molecules, atomic weight and molecular weight, of gaseous volumes and valency, and of all that is associated with or follows from these conceptions, more logical, consistent, and harmonious. What Cannizzaro did, in a word, was to throw light upon what was obscure, to introduce order where all was confused and contradictory. Hence his *Summary of a Course of Chemical Philosophy*, published in 1858, will occupy in the history of chemical doctrine a position as a classic, not less honourable than Dalton's ever-memorable *New System*. There were, of course, difficulties to be overcome, and inconsistencies to be reconciled; certain facts, indeed, appeared to be hopelessly opposed to the hypothesis which Cannizzaro sought to make the corner-stone of the edifice of modern chemistry. But these difficulties have been gradually swept away, and the very facts which at first seemed incapable of being brought into line are now seen to

afford the strongest support to the truth and universality of the theory.

The theory of Avogadro, indeed, has been approached from independent, although converging standpoints, and its position is now secured by the concurrence of independent testimony. Mathematical conceptions of the nature of gases have shown its necessity. Chemical facts, for a time, were seemingly opposed to it, and hence it was neglected and ultimately forgotten by chemists. They were, however, being driven back to it in spite of themselves; and it in no sense detracts from his merit to affirm that even if Cannizzaro had not perceived the truth, the rapidly accumulating mass of evidence would have forced others to recognise it. Indeed the substantial unanimity with which Cannizzaro's doctrine was received, immediately that it became generally known, is a proof that the time was ready for it. It is not too much to say that its effect on the minds of chemical thinkers was as profound as that described by Cannizzaro himself in the memorable lecture before alluded to, when he reminded us of Thomas Thomson's account of the impression produced upon him by Dalton's own verbal explanation of the atomic theory. To paraphrase his words: they were enchanted with the new light which burst upon their minds, and saw at a glance the immense importance of such a theory.

Hence, then, when Cannizzaro visited this country in 1872, to deliver the Faraday lecture to the Fellows of the Chemical Society, of which he has been a Foreign Member since 1862, he spoke to willing and receptive ears, and to a body of men to whom his doctrine was already an established article of their chemical creed.

Cannizzaro is a Foreign Member of many learned Societies; nearly every Academy in Europe, indeed, has

delighted to honour him. In 1889 he was elected a Foreign Member of our Royal Society, and two years later he was awarded the Copley Medal for his services to chemical theory. May he long be spared to wear the many honours he has so worthily earned, and to enjoy, in health and increasing prosperity, the respect and esteem of a multitude of friends in both hemispheres!

Addendum

Cannizzaro died at Rome on May 10, 1910, at the age of eighty-three. By his death another link between the chemistry of to-day and that of the mid-Victorian era was broken—a link which perhaps more than any other served to connect two well-defined and sharply differentiated epochs in the history of nineteenth-century chemistry.

It is not too much to say that the publication, in 1858, of his *Summary of a Course of Chemical Philosophy* created a revolution in chemical thought hardly less momentous than that which followed the appearance of Dalton's *New System*. Nay, more: it may be said to have saved the position of the atomic theory. The early 'sixties of the last century were a period of much perturbation; there was then a sort of parting of the ways. Williamson had laboured to stem the tide of infidelity, but many were unconverted, and some even hardened their hearts. We hear little or nothing to-day of the scepticism which was fashionable among the young bloods of fifty years ago. It is largely due to Cannizzaro that our faith has been strengthened and purified.

There was something dramatic in the circumstance that Cannizzaro should have passed away at the time

that all Italy was celebrating the achievements of Garibaldi and his never-to-be-forgotten Thousand in effecting the establishment of Italian unity, a cause in which Cannizzaro had himself struggled and suffered, and in which he was destined to take a share in shaping to a successful issue.

At the time of his death he was the oldest foreign member of the Chemical Society of London.—"*Nature*," *May* 19, 1910.

XVI

JULIUS THOMSEN

An Address delivered to the Fellows of the Chemical Society, 17th February 1910.[1]

Among the Danes whose names are inscribed as men of science on the eternal bead-roll of fame, that of Julius Thomsen stands pre-eminent—linked indeed with that of Oersted. It is significant of the position which Thomsen acquired in physical science, and of the respect which that position secured for him in the eyes of his countrymen, that his statue should have been erected during his lifetime and placed in the vicinity of that of Oersted in the courtyard of the Polytechnic High School of Copenhagen. Thomsen, in fact, played many parts in the intellectual, industrial, and social development of Denmark. To Europe in general he was mainly known as a distinguished man of science. By his fellow-citizens he was further recognised as an educationist of high ideals, actuated by a strong common sense and a stern devotion to duty; as an able and sagacious administrator; as a successful technologist and the creator of an important and lucrative industry based upon his own discoveries; and as a man of forceful character, who brought his authority, skill, and knowledge of men and affairs to the service of the communal life of Copenhagen.

[1] Chemical Society's Memorial Lecture.

Thomsen was a municipal councillor of that city for more than a third of a century. He occupied a commanding position on the Council, and was invariably listened to with respect. The gas, water, and sewage works of Copenhagen are among the monuments to his civic activity. From 1882 up to the time of his death he was a member of the Harbour Board of the port. In these respects Thomsen sought to realise Priestley's ideal of the perfect man—that he should be a good citizen first and a man of science afterwards.

Hans Peter Jürgen Julius Thomsen was born in Copenhagen on February 16, 1826. He was educated at the church school of St. Peter in that city, and subsequently at von Westens Institute. In 1843 he commenced his studies at the Polytechnic, and in 1846 graduated there in Applied Science, and became an assistant to Professor E. A. Scharling. Of his earliest years comparatively little is known. Thomsen, always a reserved and taciturn man, talked little about himself even to his intimate friends—and least of all about the days of his youth. It was known to a few that these days had not been smooth. Those who were best informed were conscious that to these early struggles much of that dour and resolute nature which formed a distinguishing trait in his character was due. Thomsen, indeed, began life as a fighter, and a fighter he remained to the end of his fourscore years.

In 1847 he became assistant to Forchhammer, passing rich, like Goldsmith's pedagogue, on £40 a year. Georg Forchhammer, whose earliest work dates back to the period when Berzelius was in his prime, was an active and industrious investigator of the old school, mainly in inorganic chemistry, and more particularly on problems of chemical geology and physiography.

He was a frequent visitor to this country, and was well known to early members of the British Association. Although doubtless influenced, in common with all teachers in Northern Europe, by the example and methods of Berzelius, such influence as he himself was able to exert died with him. Forchhammer attracted few pupils, and created no school, and Thomsen probably derived no inspiration or acquired any stimulus from this association. For a time Thomsen supplemented his scanty income by teaching agricultural chemistry at the Polytechnic. In 1853 he obtained a travelling scholarship, and spent a year in visiting German and French laboratories. He probably owed this scholarship in great measure to his first contribution to the literature of chemistry, namely, his memoir, "Bidrag til en Thermochemisk System" (contributions to a thermochemical system), communicated to the Royal Society of Sciences of Copenhagen in 1852, and for which he received the silver medal of the Society and a sum of ten guineas to enable him to procure a more accurate apparatus. In this memoir he sought to develop the chemical side of the mechanical theory of heat, doubtless under the influence of Ludwig Augustus Colding, an engineer in the service of the Municipality of Copenhagen, and a pioneer, like Mayer, in the development of that theory. Indeed, the Danes now claim for Colding, who had made experiments on the relation between work and heat as far back as 1842, but whose labours were practically ignored by his contemporaries, the position which the Germans assign to Mayer (see Mach's *Development of the Theory of Heat*). In 1861 Thomsen further developed his ideas in a memoir on the "General Nature of Chemical Processes, and on a Theory of Affinity based thereon."

published in the *Transactions* of the Danish Academy of Sciences. In this paper he laid the foundations of the chief scientific work of his life.

In 1853 Thomsen patented a method of obtaining soda from cryolite, so-called "Greenland," or ice-spar, a naturally occurring fluoride of sodium and aluminium, $Al_2F_6,6NaF$, found largely, indeed almost exclusively, in Greenland, and particularly at Ivigtut. It derives its mineralogical name from its ice-like appearance and ready fusibility even in the flame of a candle. It seems to have been first brought to Europe in 1794, and to have been described by Schumacher in the following year. Klaproth first showed that it contained soda, and its composition was further established by Vauquelin, Berzelius, and Deville.

Thomsen's process consists in heating a finely divided mixture of cryolite and chalk in a reverberatory furnace, whereby carbon dioxide is expelled and calcium fluoride and sodium aluminate are formed. The roasted mass is lixiviated with water, so as to dissolve out the sodium aluminate, which is then treated with carbon dioxide. Alumina is precipitated, and sodium carbonate remains in solution. The alumina is either sold as such, or converted into sulphate (so-called "concentrated alum" or "alum-cake"), and the sodium carbonate is separated by crystallisation. Both products are obtained in a remarkably pure condition, and the cryolite-soda yields excellent "caustic."

Thomsen's process, although simple enough in principle, requires considerable skill and pains in its practical execution, and most of the manufacturing details were worked out by him, or under his direction. Success largely depends upon the maintenance of a proper temperature; the decomposition begins below a

red-heat, but requires to be finished at that temperature, and care must be taken to avoid fusion or even sintering of the mass. In 1854 Thomsen obtained the exclusive right of mining for cryolite and of working up the mineral in Denmark for soda and alumina. Actual manufacturing operations were begun on a small scale in 1857, and in the following year Thomsen planned the present large factory at Oeresund, near Copenhagen, which was opened on his thirty-fourth birthday. The importance of this industry to Denmark may be seen from the circumstance that during the fifty years of its existence the firm have paid the Danish Government nearly £300,000 for the concession. Other factories were started in Germany, Bohemia, and Poland, but met with little success. The Pennsylvania Salt-manufacturing Company at Natrona, near Pittsburg, eventually obtained the right to work up two-thirds of all the cryolite mined in Greenland. From the start Thomsen took a large share in the management of the Oeresund works, and, by his energy, foresight, and skill, placed the undertaking on a sound commercial basis.

Although Thomsen died a rich man, mainly as the result of the industry he created, in the outset of his career as a teacher and a technologist his means were very straitened. He came of poor parents, of no social position or influence, and they were unable to further his inclinations towards an academical career. In 1854 he applied unsuccessfully for a position as teacher of chemistry at the Military High School in Copenhagen. During three years—from 1856 to 1859—while still engaged in developing his cryolite process, he acted as an adjuster of weights and measures to the Municipality of Copenhagen. It was a poorly paid

position, but it kept the wolf from the door. At about this period he betook himself to literature, and published a popular book on general subjects connected with physics and chemistry—somewhat in the style of Helmholtz's well-known work—entitled *Travels in Scientific Regions*, which had a considerable measure of success. He was, however, not altogether unknown even at this time as an author, since in 1853 he had collaborated with his friend Colding in producing a memoir on the causes of the spread of cholera and on the methods of prevention, which attracted much attention at the time of its appearance.

In 1859, whilst engaged in the Oeresund factory, he again applied to the authorities for a position as teacher at the Military High School, and succeeded in obtaining an appointment to a lectureship in physics, which he held until 1866. During his tenure of this office he devised his polarisation battery, which received many awards at International Exhibitions and was used for a time in the Danish telegraph service.

In 1859-60 he was "vicarius" for Scharling at the University, and in 1865 became a teacher, and in the following year Professor of Chemistry and Director of the Chemical Laboratory, a position which he retained —active to the last—until 1901, when he retired in his seventy-fifth year of age.

Before his connexion with the University, he founded and edited, from 1862 to 1878, in association with his brother, August Thomsen, the *Journal of Chemistry and Physics*, one of the principal organs of scientific literature in Denmark.

In 1863 he was elected a member of the Commission of Weights and Measures, and was instrumental in bringing about the adoption of the metric system and

the assimilation of the Danish system to that of the Scandinavian Kingdom.

In 1883 Thomsen became Chancellor of the Polytechnic High School of Copenhagen—a position which he held for about nine years. During this period he entirely changed the character and spirit of the school, and stamped it with the impress of his earnestness and industry. Under his direction, new buildings were erected and arranged in accordance with the best Continental and American models. Thomsen's administration was in marked contrast to that of his somewhat easy-going predecessor, but it is doubtful if it brought him popularity in the school. The students respected and even feared him, but his cold and unsympathetic nature evoked no warmer feeling. It was said of him by one who knew him intimately that he never learned to draw the young to him, to create in them an interest for his work, to form a school. Thomsen was a homely man, but not even in his home, says the same authority, was it possible for him to change his active, earnest, strenuous disposition—what his friends called his fighting character. But if he was always the serious master of the house, he was also its obedient servant. In reality he was a man of deep feeling, and was not without power to give that feeling expression in words, sometimes in verse, and occasionally even in music.

It was while occupying the position of Director of the Chemical Laboratory of the University that Thomsen executed the thermochemical investigations which constitute the experimental development of the ideas he had formulated in his memoir of 1861. The results of these inquiries were first made known in a series of papers published from 1869 to 1873 in the *Transactions* of the Royal Danish Society of Sciences, and from 1873

onwards by the *Journal für Praktische Chemie.* The papers were republished in collected form in four volumes (1882-1886) by a Leipzig house under the title of *Thermochemische Untersuchungen.* A summary of this experimental labour, which extended over a third of a century, was subsequently prepared by Thomsen, and published in 1905 in Danish under the title of *Thermokemiske Resultater.*

In this work he reviewed the whole of the numerical and theoretical results, to the exclusion of the greater portion of the experimental details. A translation of this volume by Miss Katharine A. Burke, entitled *Thermochemistry*, renders it readily accessible to English readers. Miss Burke has supplemented the original work by a short account, taken from the *Thermochemische Untersuchungen*, of the experimental methods employed, thereby rendering the whole more intelligible to the student. Moreover, in the English edition a partial attempt has been made to translate Thomsen's deductions into the language of modern theory based on the conception of ionisation, which, of course, was not known to science at the time the *Thermochemische Untersuchungen* was published.

It is impossible within the limits of such a notice as this to deal in detail with the immense mass of experimental material which this work embodies, and I shall not attempt, therefore, to do more than to offer a generalised statement, based mainly upon the admirable account of Thomsen's work given by Professor Brönsted to the Chemical Society of Copenhagen on the occasion of the meeting held on March 2, 1909, to commemorate Thomsen's services to science.

The conception of affinity as a cause and determining condition of chemical change is traceable in some of the

earliest efforts to co-ordinate and explain chemical phenomena. It certainly existed long prior to the time of Boyle, and was at the basis of every philosophical system after his period. We need only mention the names of Bergman, Wenzel, and Berthollet to indicate this fact. But to Thomsen belongs the credit of being the first to make the attempt to measure the relative value or strength of affinity quantitatively, and to express it numerically in definite terms which admitted of exact comparison. Thomsen's theory of affinity, as enunciated by him in his 1851 paper, was based upon his conviction that affinity could be measured quantitatively by estimating the amount of heat evolved in the chemical process. We are not immediately concerned to show whether the theory is right or wrong, or in what respect it fails. The point is that the enunciation of this principle upwards of half a century ago constituted an important step forward, inasmuch as it sought to estimate affinity in relation to a quantity which can be fixed by experiment, and is capable of expression by numbers.

In this and in the subsequent paper of which mention has been made already, he thus defines his conception of thermochemistry, and discusses, for the first time, its laws:

> The force which unites the component parts of a chemical compound is called affinity. If a compound is split up, whether by the influence of electricity, heat, or light, or by the addition of another substance, this affinity must be overcome. A certain force is required the amount of which depends on the strength of the affinity.
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> If we imagine, on the one side, a compound split up into its component parts, and on the other side these parts again united to form the original compound, then we have two opposite processes the beginning and end of which are alike. It is therefore

evident that the amount of the force required to split up a certain compound must be the same as that which is evolved if the compound in question is again formed from its component parts.

The amount of force evolved by the formation of a compound can be measured in absolute terms; it is equal to the amount of heat evolved by the formation of the compound.

Every simple or complex action of a purely chemical nature is accompanied by evolution of heat.

By considering the amount of heat evolved by the formation of a chemical compound as a measure of the affinity, as a measure of the work required again to resolve the compound into its component parts, it must be possible to deduce general laws for the chemical processes, and to exchange the old theory of affinity, resting on an uncertain foundation, for a new one, resting on the sure foundation of numerical values.

As has been proved by later theoretical and experimental investigations, the theory of thermochemical affinity is not absolutely correct at ordinary temperatures. But, on the other hand, it has been shown that a comparatively large number of processes are approximately in unison with it. Not only do they agree qualitatively, that is to say, that heat is evolved during the process, but also in the fact that the results which newer and more exact methods for estimating affinity have produced agree numerically with what would be required by the thermochemical theory. We meet here with a fundamental phenomenon which Thomsen deserves great credit for having first pointed out, but the explanation of which could not be given at the time he indicated it. It can be demonstrated theoretically that the lower we reduce the temperature and the nearer we get to the absolute zero, the more nearly is the condition for the theory fulfilled, so that at the absolute zero the theory would be found to be an exact law of nature. If it were possible to work at such low temperatures it would

be found that the evolution of heat, or the evolution of energy by the chemical process, would be an exact measure of the affinity of the process, and that under this condition the theory of Thomsen would be the accurate expression of a natural law.

But under ordinary conditions this is not so, for in reality an ever-increasing number of endothermic processes are found to occur, that is, processes which proceed with the absorption of heat. Thomsen tried at first to explain these phenomena in such a way as to keep them within his system, and he drew a distinction between a purely chemical process running conformably to his theory and a physico-chemical process which did not fall within the law. But he was gradually convinced that his theory could not be maintained in its entirety. It is to his credit that he did not seek to uphold an untenable principle, or try to defend it as did Berthelot, who almost to his dying day maintained the validity of the principle in spite of all facts.

These ideas have, in the words of Ostwald, been the scientific confession of faith of chemists throughout half a century. They have had the greatest influence on scientific thought in every branch of chemistry. It is on the basis of them that we have arrived at a theory of affinity which at the present moment is being developed into one of the most perfect chemical theories. Lastly, it is due to these ideas that the experimental material has been produced which during all time will place the name of Julius Thomsen in the first rank of men of science.

To go through this material in detail is, as I have said, impossible here. It may be stated generally that practically every simple inorganic process has been investigated calorimetrically by Thomsen, or can be

calculated by means of the calorimetric data furnished by him. In the case of organic substances, data have been given for estimating the heat of combustion of a large number of compounds. All these estimations were made by Thomsen personally, according to a prearranged plan, and in systematic succession during a period of more than thirty years. They comprise more than 3500 calorimetrical estimations. It has been truly said that this work is unique in the chemical history of any country.

Among the results of Thomsen's thermochemical inquiries which have special value for physical chemistry is his investigation of the phenomena of neutralisation, in which he shows that the basicity of acids can be estimated thermochemically, and that it can in this way be proved whether or not a point of neutrality exists. His observation that the heat of neutralisation is the same for a long series of inorganic acids, such as hydrochloric acid, hydrobromic acid, hydriodic acid, chloric acid, nitric acid, etc., supports the theory of electrical dissociation, inasmuch as this requires that the heat of neutralisation of the strong acids must in all cases be independent of the nature of the acid, because the process of neutralisation for all of them is the combination of the ion of hydrogen in the acid with the ion of hydroxyl of the base to form water. These investigations also led to the important thermochemical result that the heat of neutralisation of acids (or the heat of their dissociation) cannot be considered as a measure of the strength of the acids.

Another important result is the proof by experiment of the connexion which exists between the changes of the heat-effect with the temperature and the specific heat of the reacting substances. The first law of

thermodynamics requires the relation indicated by Kirchhoff: $\frac{dU}{dT} = C_1 - C_2$, where U is the heat-effect, T the temperature, and C_1 and C_2 are the heat capacities of the two systems before and after the reaction, and Thomsen showed by investigation of the heat of neutralisation, the heat of solution, and the heat of dilution, that this relation was satisfied. For the purpose of his inquiry, the specific heats of a large number of solutions of salts were estimated by an ingenious method, and with an exactness hitherto unattained.

Of no less importance are Thomsen's thermochemical investigations on the influence of mass. In the year 1867 Guldberg and Waage published their theory of the chemical effect of mass. But they had only verified the theory to a small extent and in particularly simple cases. They had not investigated the complete homogeneous equilibrium, because at that time no method existed for experimental investigation of such homogeneous equilibrium. Thomsen showed that the estimation could be made thermochemically. By allowing, for instance, an acid to act on a salt of another acid in an aqueous solution, the latter acid will be partly replaced by the first, which will form a salt. By mixing, for instance, a solution of sodium sulphate and nitric acid, there is formed sodium nitrate and sulphuric acid, but the process will not proceed to completion. If we have estimated the heat of neutralisation of the two acids with sodium hydroxide, the difference between these two heat-phenomena will give the amount of heat corresponding to the total decomposition of the sodium sulphate, and the heat found experimentally by mixing the two solutions will therefore show to what degree the transformation has taken place. It would be possible to

estimate thermochemically the amount of the four substances in solution, and thereby, by varying the concentration or the proportion between the initial quantities of substances, to calculate whether the Guldberg-Waage theory on the effect of mass was confirmed in this case.

Thomsen applied this method to a large number of different acids and bases, and was enabled thereby to prove the agreement with the law of the influence of mass in all the cases which he examined. He found particularly that the proportion of the one acid which remained combined with the base was constant with mixtures of constant proportion. On this basis he propounded the term *avidity*, which he defined as the tendency of the acid to unite with the base, and he showed that the avidity was independent of the concentration, and only to a small extent varied with the temperature. The term avidity has since acquired great importance, particularly since other and more exact methods for its estimation have been found. Concurrently with this, its meaning has been made clear by the theory of electrolytic dissociation.

On the basis of these estimations, Thomsen drew up the first table, based on experiments, of the relative strength of the acids, and the numbers in this table have been found to agree with the results obtained by examining the electrical conductivity of the acids.

It is worth noting that Thomsen not only produced the experimental proof of the correctness of the Guldberg-Waage theory of the effect of mass soon after the appearance of this theory, but also that he was the first to acknowledge and adopt it. It is remarkable that this work of Thomsen received so little attention, although it appeared in a widely circulated German journal, and it was not until ten years later that the law of the effect

of mass was generally recognised, as the result of the work of Ostwald and van't Hoff.

Although Thomsen's title to scientific fame rests mainly upon his thermochemical work, his interests extended beyond this particular department of physical chemistry. He worked on chloral hydrate, selenic acid, on ammoniacal platinum compounds, and on glucinum platinum chloride, on iodic acid and periodic acid, on hydrogen peroxide, hypophosphorous acid, and hydrogenium. He early recognised the importance of Mendeléeff's great generalisation, and contributed to the abundant literature it produced. His paper of 1895, "On the Probability of the Existence of a Group of Inactive Elements," may be said to have foreshadowed the discovery of the congeners of argon. He pointed out that in periodic functions the change from negative to positive value, or the reverse, can only take place by a passage through zero or through infinity; in the first case, the change is gradual, and in the second case it is sudden. The first case corresponds with the gradual change in electrical character with rising atomic weight in the separate series of the periodic system, and the second case corresponds with a passage from one series to the next. It therefore appears that the passage from one series to the next in the periodic system should take place through an element which is electrically indifferent. The valency of such an element would be zero, and therefore in this respect also it would represent a transitional stage in the passage from the univalent electronegative elements of the seventh to the univalent electropositive elements of the first group. This indicates the possible existence of a group of inactive elements with the atomic weights 4, 20, 36, 84, 132, the first five numbers corresponding fairly closely with the atomic

weights respectively of helium, neon, argon, krypton, and xenon (*Zeitsch. anorg. Chem.*, 1895, **9**, 283; *Journ. Chem. Soc.*, 1896, **70**, II. 16). He subsequently made known the existence of helium in the red fluorite from Ivigtut.

As evidence of Thomsen's manipulative ability and his power of accurate work may be mentioned his determination of the atomic weights of oxygen and hydrogen, and incidentally of aluminium. For the atomic weight of hydrogen he obtained the value 1·00825 when O = 16, which is practically identical with that of Morley and Noyes. He further made most accurate estimations of the relative densities of these gases, and of the volumetric ratios in which they enter into the composition of water. His value for the atomic weight of aluminium is nearly identical with that adopted in the last Report of the International Committee on Atomic Weights.

Thomsen maintained his interest in thermochemical problems up to the end, and was a keen and clear-sighted critic of the work which appeared from time to time during the later years of his life. This interest occasionally gave rise to controversy, and some of his latest papers were wholly polemical.

Thomsen was a pronounced atomist, and to him a chemical process was a change in the internal structure of a molecule, and the chief aim of chemistry was to investigate the laws which control the union of atoms and molecules during the chemical process. He considered that chemistry should be treated mathematically as a branch of rational mechanics. But no one insisted more strongly than he how little we really know of these questions. In summarising his theoretical ideas in the *Thermokemische Resultater*, he says, "An almost

impenetrable darkness hides from us the inner structure of molecules and the true nature of atoms. We know only the relative number of atoms within the molecule, their mass, and the existence of certain groups of atoms or radicles in the molecule, but with regard to the forces acting within the molecules and causing their formation or destruction our knowledge is still exceedingly limited." He fully realised that his own work was only the foundation on which the future elucidation of these questions must rest. "He worked," says Brönsted, "in the conviction that what we somewhat vaguely call the affinity of the atoms—their interaction, their attraction, and varying effect, etc.—follows the general laws of mechanics, and that, as he worded it, the principle that 'might is right' holds good in chemistry as in mechanics. On this foundation he hoped to be able to evolve the laws for the statics and dynamics of chemical phenomena, even although the true nature of the action is unknown."

Thomsen's merits as an investigator received formal recognition from nearly every country in the civilised world. As far back as 1860 he was elected one of the thirty-five members of the Danish Royal Society of Sciences of Copenhagen, and from 1888 until his death he was its President. In 1876 he became an Honorary Foreign Member of the Chemical Society of London. On the occasion of the fourth centenary of the foundation of the University of Upsala (created in 1477), he received the degree of Doctor of Philosophy *honoris causa*. In 1879 he was made an honorary M.D. of the University of Copenhagen. Two years later he was made a Foreign Member of the Physiographical Society of Lund, and in 1888 he was elected a member of the Society of Science and Literature of Gothenburg. In

1885 he became a member of the Royal Society of Sciences of Upsala, and in 1886 of the Stockholm Academy of Sciences.

In 1883 he and Berthelot were together awarded the Davy Medal of the Royal Society—a fitting and impartial recognition on the part of the Society of the manner in which the two investigators, whose work not infrequently brought them into active opposition, had jointly and severally contributed to lay the foundations of thermochemistry.

In the same year Thomsen was made a member of the Accademia dei Lincei of Rome, and in the following year he was elected into the American Academy of Arts and Sciences in Boston, and of the Royal Academy of Sciences of Turin. In 1887 he was made a member of the Royal Belgian Academy.

In 1886-87 and again in 1891-92 he was Rector of the University of Copenhagen. In 1888 he became Commander of the Dannebrog, and in 1896, and on his seventieth birthday, he was made Grand Commander of the same order. On the same occasion the Danish chemists caused a gold medal to be struck in his honour. In 1902 he became a Privy Councillor (Geheime Konferenz raad). In the same year he was elected a Foreign Member of the Royal Society of London.

He died on February 13, 1908, full of years as of honours, and was buried on the eighty-third anniversary of his birth and on the jubilee of the opening of the Oeresund factory. His wife, Elmine Hansen—the daughter of a farmer on Langeland—predeceased him in 1890.

XVII

THE RISE AND DEVELOPMENT OF SYNTHETICAL CHEMISTRY

THE PRESIDENTIAL ADDRESS TO THE SUTTON COLDFIELD INSTITUTE, 1892; SUBSEQUENTLY PUBLISHED IN THE *FORTNIGHTLY REVIEW*.

CHEMISTRY, as an art, dates back from the very dawn of civilisation itself; as a science it is barely a couple of centuries old. To the alchemists its pursuit was, in the main, but the pursuit of wealth. Now and again we find men among them like Thomas Aquinas, Libavius, and Glauber, who were impelled by a higher motive than the love of gold to seek for the hidden meaning of things, but the mystical tendencies of the Middle Ages were as scales to their eyes, and such devious groping for the light as they were able to make too frequently ended in utter darkness. Even in the therapeutic crudities of Paracelsus, who was sufficiently sincere in his profession as a thaumaturgist to affirm that magic was the culminating point of all human knowledge, what there was of science was summed up in the aphorism, which in fact passed as an axiom among his disciples: *Man is a chemical compound; his ailments are due to some alteration in his composition, and can only be cured by the influence of other chemical compounds.* It may be questioned whether

modern therapeutics has advanced much beyond this position.

In strict truth, it is only within the present century that men have seriously set themselves to search for the causes and conditions of chemical change. Phlogistonism, it is true, had in it the semblance of a philosophical doctrine, but it was founded on an utterly false basis, and ultimately fell and was crushed by the weight of its own absurdities. The recognition of the real nature of combustion, itself a manifestation of chemical union, paved the way towards a clearer conception of the essential nature of chemical combination; and this conception acquired a beauty, order, and harmony until then unknown to chemical teaching by the application of the atomic hypothesis as an explanation of the fundamental facts of chemical affinity. Indeed, it has become a truism to say that this conception, the fruit of patient and sustained induction, is to chemistry what the theory of universal gravitation is to astronomy. For the first time in its history chemistry was illumined and vivified by a single, consistent theory, founded on quantitative relations, and making use of definite mathematical expression; and it was at length recognised that the science must ultimately be referred to mathematical laws similar to those which had been established in regard to the mechanical properties of matter.

It in nowise detracts from the merit of Dalton to affirm that the atomic theory was a product of the age. It is certain that even if the simple, unassuming Cumberland Quaker had never lived, or that if the boarding-school at Kendal, "where youth was carefully instructed in English, Latin, Greek, and French; also writing, arithmetic, merchant's accounts, and the

mathematics," for the modest sum of ten and sixpence a quarter, had absorbed the whole of his intellectual energies, we should still have had the atomic theory. Thomas Thomson, who stood sponsor to the infant doctrine, always maintained that in the absence of Dalton, Wollaston would have been its discoverer. A younger, bolder, and more vigorous intellect than Wollaston's was also on its track, and Sweden had well-nigh secured the glory which indisputably belongs to Britain. Jöns Jakob Berzelius had clearly grasped the real significance of Richter's labours, and, before he had even heard of Dalton's theory, had dedicated his life to the determination of the stoichiometric relations on which the atomic doctrine ultimately depends. His keen mental vision saw proofs of its validity where Dalton failed to find them. He was aware also of its limitations; like Wollaston, he perceived that the mutual action of atoms requires a geometrical conception of their relative arrangement in all the three dimensions of solid extension.

Whatever may be the ultimate fate of the theory which found deliberate expression in the *New System of Chemical Philosophy*—and no one can say that it is not destined to give place to a higher and even nobler generalisation, which shall more clearly connect matter with the forces associated with it—it is certain that the ages to come will reckon it as the central, dominant conception which has actuated the chemistry of the nineteenth century. The characteristic feature of the chemistry of our time is, in a word, the development and elaboration of Dalton's doctrine; for every great advance in chemical knowledge during the last ninety years finds its interpretation in his theory.

The discovery by Gay Lussac in 1808, that the

weights of equal volumes of both simple and compound gases, when measured under like conditions of temperature and pressure, are either proportional to or are simple multiples of their empirical combining weights; and the explanation of this fact, given by Avogadro in 1811, by the assumption that all gases, when measured under comparable conditions, contain the same number of particles, as indicated by the similarity in behaviour of all truly gaseous substances under the influence of temperature and pressure,—were the first striking proofs of the validity, if not the sufficiency, of Dalton's fundamental conception. Then followed the discovery of Dulong and Petit, in 1819, that the capacity for heat of the elements is inversely proportional to their atomic weights, which finds its simplest expression in the general law that the atoms of elementary substances have the same capacity for heat; and almost simultaneously, the discovery by Mitscherlich that an equal number of atoms similarly combined ptoduces similarity of crystalline form; similarity of crystalline form being independent of the chemical nature of the atoms, and only determined by their number and arrangement. The recognition, about 1823, of the existence of Isomerism—that is, of the fact that the same elements may combine together in the same proportion and yet give rise to totally distinct compounds; Faraday's discovery in 1833, "that the equivalent weights of bodies are simply those quantities of them which contain equal quantities of electricity," or, in other words, that the atoms of bodies have equal quantities of electricity naturally associated with them; the detection by Kopp and Schröder in Germany, and by Playfair and Joule in this country, of the relations between mass and volume in solids and liquids;—these constitute so many

proofs of the soundness and comprehensiveness of the hypothesis. It does not weaken the argument that these generalisations were not invariably accepted by the leaders of chemical opinion at the time they were promulgated. Dalton himself, for example, was unable to recognise the great support his doctrine derived from the discovery of the law of volumes by Gay Lussac. Berzelius long failed to appreciate the significance of the discovery of Dulong and Petit, and although he was eventually driven to recognise the occurrence of isomerism—he, indeed, coined that very word—in the outset he stoutly maintained that the idea involved a contradiction in terms. It is true that a fuller knowledge has shown that each of these generalisations has its limitations: that all are, so to say, only first approximations to exact mathematical statement; but this circumstance has in nowise diminished the support they afford to the doctrine of Dalton, for, as Liebig has well stated, "one of the most cogent arguments for the soundness of our views respecting the existence of atoms is, that these deviations are explicable upon certain considerations attaching to the atomic theory."

Corpuscular theories are now altogether banished from certain domains of physics. Indeed, the most weighty of the attacks yet made on the atomic hypothesis have been delivered by those who are mainly occupied with the problems and abstract conceptions of energy. It is significant that Faraday, who began his scientific career as a chemist, seems to have gradually loosened his hold on the atomic theory as he became more and more absorbed in the contemplation of purely physical phenomena. On the other hand, the belief in the existence of atoms has been enormously strengthened by a profounder study of the laws which

affect the thermal relations of gases and their behaviour under pressure; the very fact that these laws are not capable of the strict mathematical expression originally given to them, finds its fullest explanation in the atomic theory; the deviations follow, indeed, as a necessary consequence of the theory, thereby affording an additional proof of the truth of Liebig's remark.

But the chemist who concerns himself mainly with considerations drawn from his own science sees in the past course and present tendency of organic chemistry the strongest proofs of the influence and value of Dalton's doctrine. All the fundamental conceptions of organic chemistry are essentially atomic. The significance of isomerism has already been referred to; it is in the domain of organic chemistry that isomerism finds its most numerous and most striking examples. As far back as 1823 Gay Lussac and Liebig clearly recognised the existence of isomerism, and also the fact that it must ultimately depend on differences in the mode in which the atoms of the constituent elements are combined. The discovery by these chemists of the identity in qualitative and quantitative composition of silver fulminate and silver cyanate was in reality one of the most momentous discoveries of the century. It was followed in quick succession by Faraday's discovery of a liquid hydrocarbon in oil gas, having the same percentage composition as the well-known olefiant gas; by Wöhler's epoch-making transformation of ammonium cyanate into urea—bodies made up of the same elements united in the same proportion, and yet possessing wholly dissimilar properties; and, lastly, by Berzelius's discovery that racemic acid and tartaric acid are identical in composition. These facts profoundly influenced chemical thought, by demonstrating that the attributes of bodies

depend not merely on the nature and number of their components, but also on the mode in which these components are arranged and distributed. Chemists, for the first time, clearly recognised that their business was not only to determine the quality and quantity of the various atoms in a compound, but also the manner in which those atoms are grouped and held together. This, indeed, is the great problem of modern chemistry, and each successive theory of the last half-century is to be valued in proportion as it has ministered to this end. The idea of atomic grouping lay at the basis of the doctrine of radicles, by means of which the chemists of sixty years ago sought to make evident that organic compounds are fundamentally similar to inorganic substances. It lay, too, at the basis of the unitary views which, with the discovery of the principle of substitution by Dumas, began to gain ground in organic chemistry. The fact of substitution, indeed, is as inexplicable as the law of multiple proportions, except upon the atomic hypothesis. The nucleus theory of Laurent, and the different type theories of the French school, and of their successors in England, all tacitly recognised the existence of atoms.

The limits of this address preclude the possibility of the attempt to show how these various theories have given place to more rational modes of viewing the structure of organic compounds. Unquestionably the two most powerful factors in determining the present character of organic chemistry have been, firstly, the recognition by Frankland in 1852 of the fact that the atom of every element is inherently endowed with a specific combining power varying within certain limits, whereby it is able to unite with a definite number of atoms of other elements; and, secondly, the hypotheses

of Kekulé and Couper, which graft themselves directly on Frankland's discovery, concerning certain peculiarities in the mode of combination of the element carbon, the organic element *par excellence*. Although nothing is known as to the real cause of valency—that is to say, of the cause which enables the atom of carbon to take to itself and hold in stable union four atoms of hydrogen or of chlorine, and no more than four, and which forbids the atom of hydrogen or of chlorine to do the same thing as regards carbon—it will be obvious that the fundamental idea of valency is essentially atomic in conception.

From this time the attempts of chemists to unravel the internal structure or *constitution*, to use the term first introduced by Butlerow, of organic compounds took a new departure. Great numbers of compounds hitherto unknown and undreamt of were prepared because theory indicated their existence. The mode of genesis and the nature of the transformations of these substances threw fresh light on the manner of the grouping of the atoms in bodies already known ; and the knowledge thus gained enabled chemists to conceive of methods by which such compounds might be synthetically prepared. Indeed, it may be safely asserted that whenever the constitution of an organic substance is clearly made out, its synthetical formation comes within the sphere of practical chemistry.

It may not be uninteresting to take a survey of the progress which has been made in synthetical chemistry during the last fifty or sixty years.

Such a survey, however rapid, will lead us into every department of the organic world. During the past half-century the chemist has succeeded in forming the active principles or characteristic products of many

plants; he has built up substances which have hitherto been regarded as made only by the very process of living of an animal; and he has formed substances which were thought to be produced only by changes in organised matter after death.

That particular day in 1828 when Wöhler first observed the transformation of ammonium cyanate into urea should be accounted a red-letter day in the history of science. This discovery virtually gave the death-blow to the doctrine that the operations which are concerned in the formation of the chemical products of the organic world are fundamentally different from those which take place in the inorganic world. Urea, as the final product of a series of tissue changes, is pre-eminently a typical product of animal life: no more characteristic example of a substance formed by the action of the so-called vital force could possibly be adduced. By demonstrating that urea can be made synthetically by ordinary laboratory processes and from substances inorganic in their origin, Wöhler proved that vital force is only another name for chemical action; and that an animal is nothing but a laboratory in which a multitude of chemical changes, similar to those which occur in our test-tubes and controlled by essentially the same conditions, is continually taking place.

Since the date of Wöhler's discovery urea has been synthetically prepared by many reactions. Thus it was obtained by Natanson by the action of ammonia on carbonyl chloride, and by Basarow by heating commercial carbonate of ammonia; its formation in this case is due to the circumstance that ordinary carbonate of ammonia, or "smelling salts," always contains a greater or less quantity of ammonium carbamate, which on heating is resolved into urea and water. These

reactions have a special interest from the circumstance that all the substances taking part in them can be formed directly or indirectly from their ultimate elements.

Associated with urea as a product of the oxidation of the nitrogenous compounds in the organism are *uric acid*, *xanthine* and *sarcine*. These bodies are met with in greater or less quantity in urine, and in certain forms of urinary calculi; and they are invariably present in blood and muscle juice. In chemical composition they differ from each other simply in the amount of oxygen they contain. Urea was first artificially transformed into uric acid by Horbaczewski. Its synthesis has also been effected by Behrend and Roosen by methods which afford evidence of the validity of the view of its constitution originally propounded by Medicus.

Closely related in chemical composition to these bodies are *theobromine* and *caffeine*, the active principles respectively of cocoa (the fruit of *Theobroma cacao*) and of coffee and tea.[1] Caffeine, indeed, is contained in a large number of vegetable products, infusions of which are used as beverages in various parts of the world. Thus, in addition to tea and coffee, it is found in maté, or Paraguay tea (the leaves of *Ilex paraguayensis*); in "guarana," the dried paste prepared from the seeds of *Paullinia sorbilis*, growing in South America; and also in the kola-nut, which is used as an article of food in Central Africa. This widespread use of caffeine-containing products is undoubtedly of great physiological import, and its interest is enhanced by the intimate relationship which is now shown to exist between caffeine

[1] It has been surmised, from the results of physiological experiments, that caffeine, the characteristic principle of coffee, differs from theine, the active principle of tea. Recent observations by Professor Dunstan have, however, conclusively demonstrated the identity of the two substances.

and the products derived from the oxidation of nitrogenous matters within the organism. Xanthine, indeed, has been recently transformed into theobromine by Emil Fischer by a method which is identical in principle with that used in 1860 by Strecker to effect the conversion of theobromine into caffeine. These bodies are closely related to *guanine*, a substance contained, as its name implies, in guano. Guanine, in fact, was transformed into xanthine by Strecker. Hence the active principle of tea and coffee could be obtained from guano, or if some method of transforming uric acid into xanthine could be devised—this transformation was at one time supposed to have been effected — caffeine could be synthetically made through the intervention of urea and uric acid from inorganic materials. The following scheme shows the successive steps of the synthesis :—

1. Carbon and oxygen give carbonic oxide.—*Priestley ; Cruikshank.*
2. Carbonic oxide and chlorine give carbonyl chloride.—*J. Davy.*
3. Carbonyl chloride and ammonia give urea.—*Natanson.*
4. Urea gives uric acid.—*Horbaczewski, Behrend and Roosen.*
5. Uric acid to be transformed into xanthine.
6. Xanthine yields theobromine.—*Strecker.*
7. Theobromine gives caffeine.—*Fischer.*

Of late years much attention has been paid to the study of the putrefaction of albuminous substances of animal origin, with the result that a considerable number of basic nitrogenous bodies, some of which are highly poisonous, have been isolated. These compounds were classed by the Italian toxicologist Selmi under the generic name of the *ptomaïnes* (πτῶμα, a corpse). They are the products of the vitality of the micro-organisms which are concerned in setting up the putrefactive change. Their discovery has greatly modified our views as to the mode of action of pathogenic organisms, and it is

now regarded as not improbable that the disturbances are due rather to the poisonous products elaborated by the organisms than to the mere presence of the organisms themselves. It ought to be stated, however, that the attempts hitherto made to isolate the toxic substances which may be formed by the growth of pathogenic organisms have been attended with only partial success. Thus Nencki was unable to detect any toxic substance among the products of anthrax, and *Staphylococcus aureus*, a common organism in abscesses, yields only a non-poisonous base. On the other hand, Brieger, to whom we owe the detection of a large number of these so-called cadaveric alkaloids, has found that the typhoid bacillus yields a poisonous substance which he has named *typhotoxine* ; and he has also discovered that the bacillus of tetanus forms a base, now called *tetanine*, which gives rise to symptoms which have a strong resemblance to those occasioned by the inoculation of the bacilli. All the ptomaines hitherto isolated are comparatively simple in composition and not very complex in chemical constitution. Some of them have been shown to be identical with bodies already known. Thus *choline* (χολή, bile), a non-poisonous alkaloid, originally found by Strecker in bile, in the brain, and in yolk of egg, and now known to be a product of the putrefaction of meat or fish, has been synthesised by Wurtz ; whilst *neurine* (νεῦρον, nerve), a derivative of brain substance, originally confounded with choline, but differing from it in composition, and in possessing intense poisonous properties, has been artificially produced by Baeyer and by Hofmann. Choline and neurine are closely related substances, and can be readily transformed into each other. Another of the so-called corpse alkaloids, *cadaverine*, has also been synthetically formed by Ladenburg.

In 1870 Schmiedeberg and Kopp isolated the poisonous principle of the fungus *Agaricus muscarius*, which they named *muscarine*. Subsequently, Schmiedeberg and Harnack obtained muscarine by the oxidation of choline, which indeed accompanies it in many fungi, and from which it differs only in containing an additional atom of oxygen. It is noteworthy that muscarine also occurs with choline among the products of the putrefaction of flesh.

It is, however, among the products of plant life that some of the most notable syntheses of modern times have been effected. *Tartaric acid*, the characteristic acid of unripe grapes, and *citric acid*, the acid which gives sourness to limes, lemons, and oranges, have each been artificially prepared. The synthesis of tartaric acid may now be effected from its elements by a comparatively simple series of steps. The first step involves the artificial formation of ordinary *alcohol*. This was effected by Berthelot in the following way. When carbon is intensely heated in a stream of hydrogen gas a poisonous gaseous hydrocarbon termed *acetylene* is produced, which is characterised by giving a red powder with certain compounds of copper. Indeed, by means of this reaction the presence of acetylene in ordinary coal-gas, and as a product of the imperfect combustion of many illuminating flames, may be recognised. This compound of copper and acetylene, under the influence of reducing reagents, may be made to yield ethylene, or olefiant gas; this gas, as Faraday showed, is soluble in strong oil of vitriol with the formation of ethyl-sulphuric acid, a substance which only requires distillation with water to convert it into ethyl (*i.e.* ordinary) alcohol.

Ordinary alcohol, under the influence of oxidising agents, gives rise to glyoxylic acid, from which, as

recently shown by Genvresse, the optically inactive modification of tartaric acid, the racemic acid discovered by Berzelius, may be formed. Racemic acid, as demonstrated by Pasteur, is a mixture of two modifications of tartaric acid which neutralise each other's optical activity. By taking advantage of certain remarkable differences in the crystalline characters of the sodium-ammonium salts formed from racemic acid, which he was the first to detect, Pasteur succeeded in separating the two constituents of racemic acid, and in showing that one of these is the particular variety which occurs in the grape and other fruits, and which constitutes the tartaric acid of commerce. Hence the racemic acid obtained from the oxidation product of alcohol only requires treatment by Pasteur's method to yield the tartaric acid of the grape.

Many attempts have been made at various times to effect the artificial formation of plant-alkaloids, such as quinine, morphine, strychnine, etc., but as yet with only partial success. There is little doubt, however, that such syntheses will be accomplished when the constitution of these complex substances is better understood. Up to the present only two alkaloids have been synthesised—one completely, viz. *conine*, the poisonous principle of hemlock (*Conium maculatum*), which has been made artificially by Ladenburg, and the other only partially, viz. *atropine*, one of the active substances in *Atropa Belladonna*.

The history of the synthesis of conine has a special interest. The naturally occurring conine obtained by Giesecke in 1827 has the power of turning the plane of polarised light to the right, whilst the conine first synthesised by Ladenburg was found to have no action on polarised light. Further research has shown that

this conine stands to the naturally occurring alkaloid in the same relation that the artificially prepared racemic acid of Genvresse stands to the tartaric acid of fruits. By combining the artificial conine with tartaric acid, it is broken up into lævo-conine and into dextro-conine, the latter of which is identical with the natural variety.

Closely related to conine is *nicotine*, the poisonous constituent of tobacco. The constitution of this body is now fairly well understood, and its synthetical formation may be confidently looked for. Its parent substance is contained in coal-tar, and in the fœtid liquid obtained by distilling bones.

A considerable number of the odoriferous principles of plants have been obtained artificially, such, for example, as *bitter-almond oil* and *oil of mustard*. Indeed, the synthetically prepared oil of mustard has largely replaced the natural product in medicine as a vesicant and counter-irritant. The sweet-smelling principle of *Spiræa ulmaria*, or meadow-sweet, is readily formed by heating a solution of carbolic acid in soda with chloroform. This substance may, as shown by Dr. Perkin, be readily transformed into *coumarin*, the crystalline odoriferous principle of the tonka bean and of woodruff. The *Maiweinessenz* used in Germany in place of woodruff in the manufacture of *Maiwein* is an alcoholic solution of artificially prepared coumarin.

Salicylic acid, which is now largely used as an antiseptic, could formerly only be obtained from oil of wintergreen (*Gaultheria procumbens*). It is now prepared on a manufacturing scale by heating a compound of soda and carbolic acid in carbonic acid gas, and the synthetic product has practically replaced the natural variety.

Vanillin, the aromatic principle of the dried fermented pods of certain orchids belonging to the genus Vanilla (Span. *vaynilla*, dim. of *vaina*, a pod), has had its constitution unravelled, with the result that it can now be synthetically prepared. Artificial vanillin is, in fact, made on an industrial scale in Germany, and threatens to supplant the natural product of Réunion and Mauritius as a flavouring agent in chocolate and confectionery. It is interesting to note that vanillin is found to be chemically related to the odoriferous constituents of cloves and all-spice, and can be prepared from these substances.

Other plant products, of which the synthetic formation may be confidently looked for, are *caoutchouc* and *oil of turpentine*; and, curiously enough, the one will be obtained through the other, and both, indirectly, from coal-tar.

The story of the artificial formation of *alizarin* from coal-tar derivatives by Graebe and Liebermann in 1868, and of its successful commercial manufacture by Perkin in this country, and by Caro in Germany, is too well known to be repeated here. The synthetic formation of alizarin created nothing less than a revolution in one of our leading industries, and completely destroyed a staple trade of France, Holland, Italy, and Turkey. Alizarin is one of the main products of the madder plant, the roots of which have been used from time immemorial for the sake of the dyes which they contain. Pliny tells us that in his time madder was well known "to the sordid and avaricious; and this because of the large profits obtained from it owing to its employment in dyeing wool and leather." Originally it was grown almost exclusively in India, Persia, and the Levant. The Moors introduced it into Spain,

whence it found its way into the Netherlands. Alsace and Avignon were long celebrated for their madder. Twenty years ago it was the most important of the natural dye-stuffs used by the calico printer and Turkey-red dyer; and the annual import of it into this country was valued at £1,250,000 sterling, the South Lanarkshire district alone consuming upwards of 150 tons weekly. The chemist has changed all this, and the cultivation of the various species of the *Rubiaceæ* for the purposes of the dyer, which has continued for thousands of years down to our own time, is now practically at an end. It is the remnant of a primeval vegetation that has displaced the vegetation of to-day.

The remarkable industrial results which followed Graebe's and Liebermann's discovery naturally roused chemists to attempt the artificial formation of a dye-stuff not less important, viz. *indigo*. This has been accomplished, independently, by Baeyer, by Heumann, and by Heymann, but by methods which as yet do not allow the artificially formed indigo to compete successfully with the natural product. Now, however, that an insight into the chemical constitution of indigo has been gained, no one can say that its synthetic formation may not at any time become commercially successful.

No synthesis of recent years has created a keener or more widespread interest than that of the *sugars*, dextrose and levulose, which has been recently effected by Emil Fischer. It is interesting not merely as an instance of the artificial production of well-known substances, but also on account of the light it is calculated to throw upon the origin and mode of formation of the sugars in the vegetable kingdom. It is not possible, without entering into details too technical in

character to be in place here, to trace the successive steps of the process by which this synthesis has been effected; but it may be interesting to note that it has been accomplished through the medium of *glycerin*, a proximate constituent of the fats, and itself capable of being formed from ordinary alcohol, which in its turn can be produced, as already stated, from its elements. Hence it is possible to indicate a method by which sugar can now be formed from its ultimate elements—that is, from inorganic sources.

The advance in every section of chemistry during this century, and especially during the latter half of it, has literally been by leaps and bounds. Although practically a creation of our own time, no branch has been more fruitful in result, in suggestion, or in possibility, than that of organic synthesis. The mere gain in the knowledge of fact is immeasurable; it is even more impossible to gauge the profound effect of that knowledge on other departments of human effort and intellectual activity.

Past history shows only too well that there is a tide in the affairs of science, as in all other things that constitute affairs of men. There are periods of ebb and flow, of advance and retrogression. But of this we are certain—in chemistry the flood has only just set in, and it is still very far from high-water mark.

XVIII

ON THE PROGRESS OF CHEMISTRY IN GREAT BRITAIN AND IRELAND DURING THE NINETEENTH CENTURY

PART I.—FROM THE BEGINNING OF THE CENTURY TO THE FOUNDATION OF THE CHEMICAL SOCIETY (1841)

THE PRESIDENTIAL ADDRESS TO THE CHEMICAL SOCIETY, 29TH MARCH 1900

ALTHOUGH Century-summaries run a risk of being regarded as flat, stale, and unprofitable, I have ventured to think that it would not be uninteresting or altogether unuseful if, at this time, I sought to indicate in the broadest possible outline what our countrymen have done during the past hundred years in gaining and extending knowledge within the special province of intellectual activity with which we, as a Society, primarily concern ourselves.

In examining the century's accounts, we naturally inquire into the nature of the balance with which it started. How much better it was felt to be than at the corresponding period of the eighteenth century may be gleaned from the circumstance that the Council of the Royal Society—actually the contemporaries of Newton—entered upon 1701 with the complaint, more petulant than dignified, that "the discouraging neglect of the great, the impetuous contradiction of the ignorant,

and the reproaches of the unreasonable, had unhappily thwarted them in their design to perpetuate a succession of useful inventions." During the years that followed, the achievements of Newton's successors, Franklin, Dollond, Maskelyne, Canton, Priestley, Hunter, made the Fellows feel that they were not wholly dependent upon the patronage of the great, although, as the battle over the lightning conductors too plainly showed, they were not altogether unmindful of the frowns and reproaches of the ignorant and unreasonable.

Let us then try to realise what was the state of things, so far as it may be thought to affect Chemistry and its pursuit, in the year 1800. To begin with, the population of these isles was less than half of what it now is. London contained only about 800,000 persons, and I need hardly say that the whole social and economic conditions of the people were vastly different from what they are to-day. In one respect, at least, the circumstances of this and that time were alike—we were at war. But the condition of the nation now is halcyon compared with what it was in 1800. England was then in such straits as she never was before. Her very existence as a nation was at stake. The harvest over a great part of the country had failed. Corn was upwards of £9 a quarter, and famine and pestilence brooded over the land. Whilst Napoleon's attempt at invasion had roused the nation to a defiant and stubborn resistance, the governing classes were torn asunder by party faction and seemingly more eager to secure political supremacy than to devise means to resist the common enemy.

Under such untoward conditions, it might well be supposed that science would languish and decay. Such, however, was not the case. It is a singular fact, and

the circumstance is not peculiar to our own history, that it is precisely during the lean years—during periods of national stress and strain—that some of our greatest intellectual triumphs have been won. In the history of Science, and especially of Chemistry, France was never more glorious than during the turmoil which preceded and followed the Reign of Terror, and in our own case never were such chemical triumphs achieved as during the gloomy period covering the dying years of the last century and the opening years of that which is now drawing to its close.

What, then, was the condition of chemical science in Great Britain in the year 1800? Who were its cultivators and what were they doing? Black had died in the preceding November; Priestley, who had suffered "the slings and arrows of outrageous fortune" in vainly buffeting with the times, had been settled some six years on the banks of the Susquehanna, occupying himself with his beloved theology and in futile efforts to resuscitate the dying doctrine of phlogiston. For, in 1800, phlogistonism had only been scotched in this country. Mr. Richard Kirwan, President of the Royal Irish Academy and Inspector-General of His Majesty's Mines in the Kingdom of Ireland, had, it is true, some years previously tried conclusions with the French chemists in his *Essay on Phlogiston*, which had been translated by Mme. Lavoisier, in order to be refuted, point by point, by Lavoisier, Morveau, Laplace, Monge, Berthollet, and Fourcroy. The new theory was, in fact, indebted to this opposition for some of the strongest proofs on which it is founded. The nimble-witted Irishman found he was no match for this formidable battalion. He confessed himself vanquished, and, as he wrote to Berthollet, laid down his arms. Rather, he might have

said, he went over to the enemy, for during the few years of activity which remained to him he preached the new doctrine with all the zeal of the convert, albeit a little restive at times under the tyranny of the nomenclature it imposed upon the chemical world. National prejudice, and the authority of Priestley and Cavendish no doubt retarded its general adoption in this country. Cavendish, at the close of the last century, was in his sixty-ninth year; he had ceased to engage in chemical inquiry, although still keenly interested in its progress. James Watt, who played so notable a part in the events which led to the discovery of the composition of water, and younger by some five years than his quondam rival, was no longer actively occupied with chemical speculation. Wollaston, as dispassionate and hardly less reserved than Cavendish, was in his thirty-fourth year, struggling to get together the medical practice which his singularly cold and taciturn manner forbade him to expect. John Dalton, also in his thirty-fourth year, had just resigned his post as tutor at the Manchester Academy, where, as the Trustees were pleased to report, "he had uniformly acquitted himself to their entire satisfaction in the province of mathematics, natural philosophy, and chemistry," on the spacious salary of £80 a year, and was now looking out for pupils in those branches of learning at the rate of two shillings a lesson. Thomas Thomson was twenty-seven; Andrew Ure, of dictionary fame, was twenty-two—the same age as Humphry Davy, who was still at the Bristol Pneumatic Institute, where he had published his *Researches, Chemical and Philosophical, chiefly concerning Nitrous Oxide*. William Thomas Brande was a lad of twelve; and little Michael Faraday a child of eight, who lived in a mews near Manchester Square, keeping body and

soul together on the weekly loaf he got from public charity, and whiling away his leisure in playing marbles in Spanish Place.

Of course, the results of the more important chemical investigations of the period were laid before the Royal Society, and were published in the *Philosophical Transactions*, and subsequently in *Nicholson's Journal* or *Tilloch's Philosophical Magazine*. The Society at that time was, and had been for the previous twenty-two years, under the benevolent despotism of Sir Joseph Banks. The other officers were Wegg, Planta, Gray, and Layard. The meetings were held in the evening at eight o'clock, and, of course, for the most part by candle-light, in one of the large rooms in Somerset House, or Somerset Place as it was then termed. The Society had moved, in 1780, from Crane Court into apartments provided for them by the munificence of their patron, George III., and the skill of his architect, Sir William Chambers, which habitation, in the language of the Council, "it was hoped would confer on them an external splendour in some measure proportional to the consideration in which they had been held for more than a century." Hard by was the Crown and Anchor Tavern, where the Royal Society Club held its pious orgies; the expense of the dinner was limited to five shillings, and black puddings were a standing dish. It is evident that the drowsy philosophers did no violence to their digestion by a too prolonged attention to the intellectual fare of the evening, for even a comparatively short communication not unfrequently occupied two if not three sittings. The President, when in town, lived in the then fashionable neighbourhood of Soho Square. Cavendish's town house was at the corner of Montague Place and Gower Street, and from here he might be descried, in a faded

violet court-dress, frilled shirt-front and wrists, with a greatcoat of grayish-green, and a three-cornered cocked hat over his knocker-tailed periwig, slouching along, one hand behind his back, on the way to his library in Dean Street, Soho. Wollaston lived in Hunter Street, Brunswick Square, and subsequently in Dorset Street, Manchester Square; Smithson Tennant oscillated between his farm at Cheddar and his chambers in the Temple; and Charles Hatchett resided in the rural district of Hammersmith. Other notable chemical workers at this period were William Henry, of Manchester, William Haseldine Pepys, and Richard Chenevix.

There were, of course, other publishing agencies at work, with aims similar to those of the Royal Society, such as the Royal Society of Edinburgh, which grew out of a Society founded in 1731 by the leading medical men of that city; and the Royal Irish Academy, founded in 1782, and of which Kirwan was President for some years. Many of the provincial towns, too, had their Literary and Philosophical Societies, but with the exception of that at Manchester, to which the Henrys and Dalton were frequent contributors, their *Proceedings* and *Transactions* contain few important chemical memoirs during the first years of the century.

Theoretical chemistry was taught in the Universities, and in some of them, following the example set by Black, by means of experimental courses of lectures. At Oxford there had been Beddoes, but his political opinions were distasteful to the University, and he had resigned. At Cambridge there were F. J. Wollaston and Farish. Hope, who first made known the existence of strontia, lectured at Edinburgh; Robert Cleghorn at Glasgow; and Dr. French, who is credited with the

saying that "Humphry Davy was a verra troublesome person in chemistry," taught at Aberdeen. Moreover, almost every large town had one or more teachers—men of the type of Varley, Garnett, and Aikin—who gave lessons at their private houses. Faraday relates how, during his bookbinding days, his master allowed him to go occasionally of an evening to hear the lectures delivered by Mr. Tatum, at his house in Dorset Street, of which he obtained knowledge by bills in the streets and shop windows. The hour was eight o'clock in the evening, and the charge was one shilling. More ambitious or more self-important people than Mr. Tatum announced their prospective courses among the *Miscellanea* of Tilloch's or Nicholson's Magazines. Thus, Dr. Beddoes informs the world that his "Lectures are to be calculated for both sexes and different ages; and that there may be little chance of exclusion by reason of narrow circumstances, the subscription is fixed at one guinea; but unless fifty persons shall have entered their names by the 31st of March, the Lectures will not go on, as without a tolerably numerous audience, Dr. B. thinks he could bestow his time in a manner more advantageous to the public."

The principal manuals of the time were Kerr's translation of Lavoisier's *Elements*; Chaptal's *Elements*, the standard text-book on applied chemistry; Parkinson's *Chemical Pocket-book or Memoranda Chemica*; Heron's *Elements*; Brisson's *Physical Principles of Chemistry*; Accum's *System of Theoretical and Practical Chemistry*; and Marcet's *Conversations*.

The first two decades of this century are unquestionably the most momentous and the most brilliant of any period in the history of chemistry in our country. They witnessed the establishment of the fundamental laws of

chemical combination and the atomic theory; the discovery of the so-called gaseous laws and of the real nature of the atmosphere; the application of voltaic electricity as an analytic agent; the isolation of the metals of the alkalis and of the alkaline earths; the determination of the chemical nature of the halogens; and in the discovery of tantalum, palladium, iridium, osmium, and rhodium a considerable addition to the list of the metallic elements. These, in 1802, were only twenty-three in number, as against about sixty at the present time.

A number of new compounds were brought to light or identified during the same period—such as fulminating mercury and fulminating silver, carbonic oxide, acetylene, phosgene gas—for the most part substances deemed at the time of their discovery to be merely chemical curiosities, incapable from their very nature of being turned to useful account, but all of which are now of important practical application. The same period witnessed the invention of the reflecting goniometer and of the miner's safety lamp. It saw the establishment of gas-lighting, and with it the creation of an industry which has exercised a profound effect on the development of the chemical arts. It saw, too, the first attempt to control, by the aid of chemical analysis, the hygienic character of the drinking water supplied to a large community. The statement, in 1808, of Dr. Andrew Ure, who became Professor of Chemistry in Anderson's College in 1818, that the water from the wells in Glasgow, which were then the chief sources of supply, contained "a surprising quantity of heterogeneous matters in solution," paved the way for the action of Telford and Robertson Buchanan in bringing in "the inexhaustible supply from the river Clyde by means

of pipes and steam-engines." In the *Philosophical Magazine* for 1808 we read that this was the first occasion on which the whole of a public supply was filtered "by means of reservoirs constructed for the purpose. . . . This salutary process is effected by making the water filter through sand and gravel from the large reservoir into which it is first elevated by the steam-engine into a second reservoir posited a little lower and from which the conveying-pipes receive their supply." It is not often that Glasgow is willing to take a lesson from Paisley, but it is interesting to read that a Paisley 'buddy' first practised the filtration of water intended for public supply, and it is satisfactory to learn further that "this public spirited adventurer was amply remunerated for his expenditure."

The promulgation of the atomic theory and the importation of the ideas of constancy of chemical composition and of definite numerical ratio gave an immediate impetus to analytical chemistry, of which the operations were at length seen to be subject to a numerical check and control hitherto unknown and barely suspected. Whilst the reflecting goniometer, on the one hand, served to define the geometrical form of a substance and to establish the constancy of that form with a precision unattainable by the graphometer of Carangeau, the balance, on the other, rendered more and more perfect, in response to the demands of the chemist upon the mechanician, in order to satisfy the conditions imposed by the fundamental laws of Dalton's hypothesis, served to determine its exact atomic composition. The first use to which Wollaston, in 1810, put his reflecting goniometer was to measure with exactitude the primary angles of calcspar, the composition of which had been accurately ascertained by Richard Phillips in 1803. A

number of compounds indeed had their real nature and composition established during this period, and in the light of the atomic theory. In fact, the memoir by Thomson on the Oxalates (*Phil. Trans.* 1808), and that by Wollaston "On super-acid and sub-acid Salts" (*Phil. Trans.* 1808), were of the greatest service as proving the validity of Dalton's theory of chemical combination. Smithson Tennant analysed emery; Hatchett ascertained the composition of magnetic pyrites; Chenevix that of the arseniates of copper and iron, and of bournonite; Thomson that of sodalite, fluorspar, and allanite.[1] Edward Clarke, of Cambridge, analysed petalite, gehlenite, the purple of Cassius, and examined the newly-discovered metal cadmium and its salts.

It would be impossible to exaggerate the influence which the discoveries of the first twenty years of the century exerted on the spirit of the age. They seemed, indeed, to presage a new era—the coming of a glorious day of which the dawn had already begun. No generalisation was more opportune in its announcement than that of Dalton; it was indeed, to use the common phrase, "in the air"; the teaching of a long succession of philosophers and the experimental labours of many workers had paved the way for its acceptance, and it is practically certain that had Dalton not formulated it, Wollaston or Berzelius would have done so. As it was, each of these distinguished men was almost immediately able to supply the strongest experimental proof of its soundness. Wollaston, from the outset, was well aware of the limitations of Dalton's explanation of the facts of chemical combination; and he recognised that the arithmetical relation alone of the proportions of

[1] Allanite, in addition to cerium, contained, as he supposed, a new metal which he proposed to distinguish by the name of "junonium."

elementary atoms is insufficient to explain their mutual action. As he says, "we shall be obliged to acquire a geometrical conception of their relative arrangement in all the three dimensions of solid extension." How Berzelius received the news of Dalton's discovery he has himself related in his memorable paper in *Gilbert's Journal.* This view of the combinations of bodies, he says, "appeared capable of illustrating so greatly the doctrine of affinity, that the confirmation of Dalton's hypothesis seemed to be the greatest step that chemistry, as a science, would have made during the whole time of its existence." How it was confirmed by Berzelius it is not necessary here to state, nor need we set out in detail how the whole course of modern chemistry, however complex and many-sided it may seem, is really one vast elaboration of the atomic theory: in fact, as Liebig has said, all our ideas are so interwoven with that theory that it is difficult to carry ourselves back to the time when it did not exist. Dalton was the first recipient of one of the Royal medals which the Sovereign, on the recommendation of the Royal Society, annually bestows, and no more just award was ever made. Davy, in making known, as he said, "this first testimony of royal benevolence to science," stated that this discovery of the simple principle, universally applicable to the facts of chemistry, laid the foundations for future labours respecting the sublime and transcendental parts of the science of corpuscular motion. Dalton's merit, in this respect, resembles that of Kepler in astronomy. "The causes of chemical change are as yet unknown, and the laws by which they are governed; but in their connection with electrical and magnetic phenomena there is a gleam of light pointing to a new dawn in science; and may we not hope that in another

century, chemistry having, as it were, passed under the dominion of the mathematical sciences, may find some happy genius, similar in intellectual powers to the highest and immortal ornament of this Society, capable of unfolding its wonderful and mysterious laws."

Volta's famous letter of 20th March 1800 to Sir Joseph Banks, was as the "order of the day" of a great leader. No more momentous document was ever given to the scientific world. "The voltaic battery," said Davy, in one of his characteristically happy phrases, "was as an alarm-bell to experimenters in every part of Europe; and it served no less for demonstrating new properties in electricity, and for establishing the laws of this science, than as an instrument of discovery in other branches of knowledge; exhibiting relations between subjects before apparently without connection, and serving as a bond of unity between chemical and physical philosophy."

It is not necessary here to dilate upon what this "instrument of discovery" in the hands of our countrymen has done for the development of chemistry. April 30, 1800, when Nicholson and Carlisle first effected the decomposition of water by electrolysis, is a red-letter day in the history of our science. The *Transactions of the Royal Society*, *Nicholson's Journal*, and the *Philosophical Magazine* in the first years of the century show with what ardour and with what a lively expectancy "the new galvanic apparatus of Mr. Volta" was applied.[1]

Although even as far into the century as the date of Waterloo the theory of the compound nature of

[1] This at the time was usually constructed of plates of silver and zinc, with intervening pieces of cloth. "Most of our philosophers," we are told, "have used half-crowns for the silver plates," and zinc could be bought at 8d. per lb. at the White Lion in Foster Lane, and cast in moulds of chalk.

water was regarded by some as "nothing more than a fanciful conjecture" and "a bare assertion without any proof whatever," there can be no question that Nicholson and Carlisle's discovery was of the greatest service in affording what was then, and long subsequently, looked upon as one of the strongest analytical proofs of the validity of the fact. But in their effect upon the popular imagination these discoveries altogether paled before that of Davy. The isolation of potassium is, in reality, one of the most dramatic episodes of an epoch fruitful in striking incidents. The remarkable nature of the substance itself, its intense chemical activity, its significant chemical relationships, and the astonishing and far-reaching possibilities which its very existence seemed to open out, were all calculated to impress the minds of *virtuosi* and vulgar alike. Davy's triumph, indeed, was regarded as the realisation of a confident anticipation that the most important discoveries in chemistry were about to be made by the help of galvanism—an anticipation which found expression in almost every issue of the scientific journals of the time. There is little wonder, therefore, that a succession of scientific events of such magnitude as those which characterised the opening years of this century should have powerfully excited the imaginations of all cultured persons, and that optimists in every country, under the sway of ideas which made up the social and political philosophy of the time, should look forward in sure and certain hope to the immediate advent of a new era—to the coming of a regenerated humanity in a golden age. Thus Christopher Girtanner, of Göttingen, whose name lives only in connection with the fact that he was the first German chemist to appreciate and teach the new doctrines of the French

School, was convinced that in the nineteenth century the transmutation of the metals would be generally known and practised. "Every chemist," he says, "every artist, will make gold; kitchen utensils will be of silver, and even gold. . . . There will be no other riches than natural riches — the productions of the soil; artificial riches, such as gold, silver, and paper money, will vanish in the hands of those who have accumulated them. What a revolution in society! Every enlightened chemist, however, will agree with me that this revolution is not only probable, but at no great distance." Unhappily for Dr. Girtanner's reputation as a prophet, the social revolution he foretold may not be reckoned among the achievements of the nineteenth century. Enlightened chemists still appear to have an eye to artificial riches, not scorning even paper money; and although, unfortunately, the precious metals do occasionally vanish in our hands, it is not because we convert them into kitchen utensils.

Even the sober-minded Faraday was not insensible to the visions thus created. In the course of his lectures on the metals, given in 1818 to the City Philosophical Society, he says, "to decompose the metals, then to reform them, to change them from one to another, and to realise the once absurd notion of transmutation, are the problems now given to the chemist for solution. Let none start at the difficult task, and think the means far beyond them. Let us but look to the means which have given us these bodies, and to their gradual development, and we shall then gain confidence to hope for new and effective powers for their removal from the elementary ranks. . . . Consider the improvement when, by a variety of manipulations, the early chemist of the last century separated a small quantity of a metallic

substance from five or six other bodies, where it existed in strong combination, and then passed to the perfection of these means as exhibited in the admirable researches of Tennant and Wollaston; lastly, glance but at the new, the extraordinary powers which the chemist of our own nation put in action so successfully for the reduction of the alkalies and earths, and you will then no longer doubt that powers still progressive and advanced may exist and put at some favourable moment the bases of the metals in our hands."

It is interesting to note that even as far back as 1806 an effort was made to form a Chemical Society independent of the Royal Society, to be called the London Chemical Society, but it received scant encouragement from persons in high places, and few chemists of note joined its ranks. Sir Joseph Banks, indeed, frowned down upon all such attempts. "I see plainly," he once said, "that all these new-fangled associations will finally dismantle the Royal Society, and not leave the old lady a rag to cover her." And the frown of the masterful old President meant social ostracism to all who chose to disregard it. But, unlike his predecessor, Sir John Pringle, who, on a certain memorable occasion, confessed to George III. that he was unable to reverse the laws and operations of nature, and was then told he had better resign, Banks, if he could not reverse, could at least control and modify a natural tendency, and with the aid of Wollaston, Davy, Hatchett, and Brande he managed to keep chemistry for a time almost exclusively under the cloak of "the old lady."

Priestley died on 9th February 1804. Although he lived long enough to have read of Dalton's explanation of the laws of chemical combination, as explained in

Thomson's *System*, and although he must have heard of the electrolytic decomposition of water by Nicholson and Carlisle in 1800, nothing apparently could shake his conviction of the essential and inherent truth of the conception of phlogiston. The last of his published writings was his memoir on "The Doctrine of Phlogiston established, and that of the Composition of Water refuted." Tolerant and receptive as he was in all other matters of opinion, and especially in matters of religious belief, he seemed utterly incapable of appreciating the real significance of the rapidly accumulating mass of facts, or of drawing any correct inferences from them.

Cavendish, "le plus riche de tous les savants et probablement aussi le plus savant de tous les riches," died in 1810. His work belongs to a preceding age, for on the downfall of phlogistonism he had ceased to occupy himself with chemical pursuits, and it must ever remain doubtful how far he gave in his adhesion to the new doctrine to which his own cardinal discovery so largely contributed.

In the year 1815 appeared in Thomson's *Annals of Philosophy* (vi. 321) an anonymous essay "On the Relation between the Specific Gravities of Bodies in their Gaseous State and the Weights of their Atoms," which the author states he submits "to the public with the greatest diffidence; for, although he has taken the utmost pains to arrive at the truth, yet he has not that confidence in his abilities as an experimentalist as to induce him to dictate to others far superior to himself in chemical acquirements and fame." With this modest preamble the author proceeds to discuss certain consequences which seem to him to follow from the doctrine of volumes as first generalised by Gay Lussac. The paper is remarkable, in spite of its errors and crudities,

for its originality and lucidity. Its author was eventually discovered to be William Prout, then a young medical student, who lived to become eminent as a physiological chemist. His work thus tentatively put forward has become classical as containing or, rather, suggesting, the hypothesis that the atomic weights of the elements are multiples of that of hydrogen. For it is noteworthy that the paper nowhere contains in explicit terms the statement of what is now known as Prout's law. Its main purpose was to set out more clearly than had hitherto been perceived the relation between the doctrine of gaseous volumes and of atoms, and it is only incidentally remarked that he "had often observed the near approach to round numbers of many of the weights of the atoms," and that "Dr. Thomson appears also to have made the same remark." He further points out that it would appear from his tables "that all elementary numbers, hydrogen being considered as 1, are divisible by 4, except carbon, azote, and barytium, and these are divisible by 2, appearing, therefore, to indicate that they are modified by a higher number than that of unity or hydrogen. Is the other number 16 or oxygen? And are all substances compounded of these two elements?" Prout's "law" has been frequently so stated as to imply that all substances are "compounded" of hydrogen. But, as will be seen, his own words convey no such idea.

It is not improbable that the idea that all atomic weights were actually integers would have attracted little, if any, attention, had it not been adopted by Thomson and given currency by means of his *System of Chemistry*, then, and for some years subsequently, one of the leading British text-books.

Smithson Tennant, Professor of Chemistry in the

University of Cambridge, a man who, it was said in Johnson's well-known words, is "to be mentioned with reverence rather for the possession than the exertion of uncommon abilities," died in 1815—accidentally killed whilst riding in the neighbourhood of Boulogne. Born at Selby in 1761, and a pupil of Black, he was early attracted to the study of chemistry, which he continued to prosecute at Cambridge as a Fellow Commoner of Christ's College. His private fortune exempted him from the necessity of following actively any profession. Although a man of wide reading, and of a quick and active mind, we are told that there was a singular air of carelessness and indifference in his habits and mode of life, his manners, appearance, and conversation being the most remote from those of a professed student. "His college rooms exhibited a strange, disorderly appearance of books, papers, and implements of chemistry, piled up in heaps, or thrown in confusion together." He was fond of foreign travel, and in the course of one of his journeys made the acquaintance of Scheele, for whom he had a high admiration. He was also on terms of friendship with many of the leading French chemists of his time. Chemistry in Germany at this period was a mixture of science and credulity, the philosopher's stone was spoken of with respect, and Tennant relates that he received from a man of science and character an introduction to a person who was reputed to be in possession of that treasure, with whom he conversed in Latin, and who exhibited to him the mysterious powder, "enlarging upon its transcendent qualities with much pomp, and in flowing and sonorous periods."

Tennant is best known for his analysis of carbon dioxide, his work on the nature of the diamond, and on

the injurious effects of magnesia in agriculture, his chemical examination of emery, and his discovery of iridium—so-named from the various colours of its solutions, and osmium—which owes its name to the smell of certain of its oxides. In the course of an inquiry by a Committee of the Royal Society, formed at the request of the Government to investigate the danger that might attend the general introduction of gas-lighting in the metropolis, he made the observation that flame will not pass through small tubes, a discovery which in the hands of Davy led to the invention of the safety lamp.

The third decade of the century saw the rise of Faraday, Graham, and Edward Turner. Faraday had thrown up bookbinding in 1813, and attached himself to the Royal Institution, getting his "baptism of fire" in working upon Dulong's newly-discovered nitrogen chloride, which was then occupying Davy's attention. He assisted Davy, too, in his work on iodine, on the torpedo, on the nature of the diamond, on euchlorine, and on the chlorides of phosphorus; and in his memoir on "Some combinations of Phosphorus" the master pays a well-merited tribute to the "accuracy and steadiness of manipulation of his assistant."

Although Davy had propounded his proofs of the elementary nature of chlorine as far back as 1810, and although these had been strengthened by his work on iodine in 1814, his doctrine was not universally accepted, the Scotch teachers in particular, notably Murray and Ure, even as late as 1818, making repeated and determined attempts to subvert it. Faraday did yeoman service in repelling the attack, which eventually degenerated into a wordy warfare between Murray and John Davy.

With the beginning of 1820 Faraday may be said to have served his apprenticeship to science. He had already contributed some two-score notices and papers to the *Quarterly Journal of Science*, and had published his remarkable work with Stodart, a surgical instrument-maker, on wootz and other alloys of steel. He had discovered tetrachlorethylene, perchlorethane and ethylene di-iodide, an account of which constitutes his first paper in the *Philosophical Transactions*, and shortly afterwards he described, with Richard Phillips, a new compound of chlorine and carbon, first observed by Julin, of Abo, and now known as hexachlorobenzene. In 1823 appeared his memorable paper "On fluid chlorine," and immediately afterwards were liquefied hydrogen chloride, sulphur dioxide, carbon dioxide, sulphuretted hydrogen, euchlorine, and nitrous oxide. It should be stated, however, that Monge and Clouet had already condensed sulphur dioxide, probably before the year 1800, and that Northmore, with the assistance of Accum, had obtained liquid chlorine in 1806 by compression at ordinary temperatures (*Nicholson's Journal*, 12 and 13). Northmore's description of the properties of liquid chlorine is so explicit that it is remarkable that the significance of his observation should have escaped notice. The name and fame of the Royal Institution are indissolubly connected with the work so auspiciously begun by Faraday, and with the liquefaction and solidification of air and hydrogen by my predecessor in this chair this chapter in the history of physical science closes with the closing years of the century. It will ever be accounted among the triumphs of the nineteenth century that it has effected the removal of those artificial boundaries—the hard-and-fast lines of demarcation—implied by terms which have now no

other rational meaning than as denoting states of physical aggregation. Nor will it be forgotten that it is largely to the labours of our countrymen, Dalton, Faraday, Andrews, and Dewar, that this achievement is to be ascribed.

In 1825 Faraday discovered benzene. He found it among the products furnished by the operations of the Portable Gas Company. Although he ascertained that the density of its vapour was 39, hydrogen being 1, the significance of that circumstance was not perceived at the time. The work of Ampère and Gay Lussac, in fact, was not really understood or appreciated at this period, and the conception of Avogadro lay dormant. "With regard to the composition of this substance," wrote Faraday, "my experiments tend to prove it a binary compound of carbon and hydrogen, two proportionals of the former element being united to one of the latter." What Faraday's bicarburet of hydrogen has developed into need not now be told. The work which has accumulated round this single substance during the seventy-five years which have elapsed since it has been known constitutes one of the most astonishing records of intellectual and industrial activity of which history has any record.

Faraday's next contribution to aromatic chemistry consisted in a study of the action of sulphuric acid on naphthalene, or "naphthaline" as it was then termed—a name which we owe to Dr. John Kidd, who lectured on chemistry at Oxford in the early part of the century, and who accurately described the main properties of the hydrocarbon, which appears to have been first observed in 1819 by Alexander Garden, of Old Compton Street, Soho, where the London Chemical Society had its habitation. The products obtained by Faraday are

now known as α- and β-naphthalenesulphonic acids, the salts of which he described. It is noteworthy that in this year (1826) the Friday evening discourses at the Royal Institution were instituted by Faraday, with a view, as he says, to facilitate our object of attracting the world and making ourselves, with science, "attractive to it," and that this work on naphthalenesulphonic acid was the subject of the first chemical discourse of the series. Unpromising as it might seem, we may be quite sure that Faraday made it attractive.

Although Faraday continued to occupy himself at times with chemical subjects, even to the end of his tenure of the Fullerian professorship at the Royal Institution, he gradually became more and more absorbed in those memorable physical researches with which his name will ever be associated. The more important portion of his chemical work was practically all done before the end of 1826, and, curiously enough, one of the last inquiries with which he concerned himself in that year serves to connect him with Graham, whose services to science began in that year. It happened that Faraday, in 1823, had made some observations, perhaps among the earliest ever made, which served to establish what we now know as the transpiration of gases, but their accuracy had been doubted by Davy, who, three years later, repeated them. Davy's experiments on this subject constitute, in fact, the last ever performed by him in the laboratory of the Royal Institution. Faraday independently repeated the observations of 1823 and established their validity.

Graham's chief work, of course, lay in the borderland between physics and chemistry. The finite extent of the atmosphere; the absorption of vapours by liquids; the law of the diffusion of gases; the motion of gases;

the diffusion of liquids; osmotic force; on liquid transpiration; on molecular mobility of gases; the absorption and dialytic separation of gases by colloid septa; on occlusion of gases; hydrogenium — however different may be the titles of his papers, all his work centred round conceptions of atoms and molecules and their motions. Of his contributions to pure chemistry, the most important was that "On the arseniates, phosphates, and modifications of phosphoric acid"—the first of his papers sent to the Royal Society, to which it was communicated by Edward Turner, whom he succeeded a few years later in the Chair of Chemistry at University College. This memoir established the existence of three modifications of phosphoric acid—ortho-, pyro-, and meta-phosphoric acid. The first, or ordinary phosphoric acid, has been known for at least three centuries; pyrophosphoric acid was discovered by Clark, whom Graham succeeded as Lecturer on Chemistry at the Mechanics' Institute, Glasgow; metaphosphoric acid was now made known for the first time. The paper is memorable from the circumstance that it first definitely indicated the idea of *basicity*.

Graham's memoirs on the part played by water as a constituent of salts, and on the constitution of salts, are logically connected with and dependent on this paper. Indeed, nothing is more remarkable in Graham's work than the continuity of idea which pervades it. Just as the long succession of memoirs on the molecular movements of gases and liquids gave a definiteness and precision to the conception of the intestinal motions in matter, so his work on acids and salts served to establish the constitutional analogy between these substances.

Edward Turner was born in Jamaica in 1798, and

after studying medicine at Edinburgh, passed over to Göttingen, where he worked under Stromeyer. In 1824 he became a lecturer in Edinburgh, and here published an *Introduction to the Study and Laws of Chemical Combination*, which he subsequently worked into his excellent *Elements of Chemistry*, one of the standard text-books of the time, and by which he is mainly remembered. In 1828 he was made Professor of Chemistry in the newly-created University of London, now known as University College, and died in 1837 in the fortieth year of his age. Among his pupils was the late Sir Richard Quain, who described him as a lucid lecturer and a good analyst. Turner was indeed an excellent manipulator, and his analytical and determinative work was of a high order. He is especially to be remembered for his determinations of the atomic weights of lead, chlorine, silver, barium, mercury, nitrogen, and sulphur; they were the first atomic weights to be accurately ascertained by a British chemist, and are worthy to be ranked with the determinations of Berzelius. Turner's numbers were not only valuable as constants, but they were of theoretical importance as directly impugning the validity of Prout's hypothesis, which, mainly on the authority of Thomas Thomson, who had sought to support it by what is perhaps the weakest experimental evidence ever put forward, was part of the current doctrine of the time in this country. On Thomson's work Berzelius had passed what is probably the most scathing criticism he ever uttered: "This work belongs to those few productions from which science will derive no advantage whatever. Much of the experimental part, even of the fundamental experiments, appears to have been made at the writing desk; and the greatest civility which his contemporaries can show

its author, is to forget that it was ever published" (*Jahresber.*, 1827, 77).

Turner pointed out (1) that the atomic weights hitherto commonly used by British chemists had been adopted without due inquiry, and that several of the most important ones were erroneous, and (2) that the hypothesis that all equivalents are multiples of a whole number of the equivalent of hydrogen was inconsistent with the state of chemical knowledge at the time, being at variance with experiment. "Under such circumstances, Dr. Prout's hypothesis, as advocated by Dr. Thomson—that all atomic weights are simple multiples of that of hydrogen—can no longer be maintained. I grant most willingly that a system of whole numbers, considered as moderate approximations, may, with advantage, be retained for the use of medical men, students, and manufacturers; but as the strict representative of scientific truth, applicable to all the purposes of science, this hypothesis is at present untenable. Let me not, however, be misunderstood: I mean simply to affirm that the experiments by which it has been attempted to prove the truth of this hypothesis are inaccurate; I may go further, and declare it to be not only unsupported by evidence, but to be at variance with the most exact analytic researches which have been conducted. I deny not that some simple relation subsists among atomic weights, and that their ratios may possibly be expressed by some series of numbers; but at present no one has assigned any physical cause for the existence of such a relation; no such relation has hitherto been discovered; nor, as it appears to me, has analytical chemistry attained that degree of perfection which can justify any one in finally asserting or denying its existence." The position thus taken up by Turner is

precisely that to which Stas arrived half a century later, after an inquiry which will ever remain a model of analytical skill.

William Hyde Wollaston died on 22nd December 1828, at the age of sixty-two; Thomas Young on 10th May 1829, aged fifty-six; and Humphry Davy on 29th May 1829, aged fifty-one. The passing away of three such men in the ripeness of their intellectual vigour and in such quick succession profoundly affected the whole scientific world. They had been colleagues during life, each holding high office in the Councils of the Royal Society, and were, with Dalton, the brightest ornaments of one of the most glorious epochs in the history of physical science in this country. At present we are only concerned with Wollaston and Davy. On Davy's triumphs, on his mental powers, and on the characteristics of his genius it is unnecessary now to dwell; with the exception of that of Dalton, there is probably no personality in the history of British chemistry with which we are more familiar. Wollaston is less well known; his achievements, indeed, were less calculated to strike the popular imagination than those of his great contemporaries. But this silent, austere man, who lived in and for his laboratory and his library, his only relaxation being an occasional evening among his fellows at the Royal Society Club, or at the gatherings of the Royal Society, had a wonderfully clear intelligence and an astonishingly penetrative insight. No man ever more justly earned the title of philosopher. He was not so prolific or so diffuse as Priestley, but his learning was infinitely deeper and broader, and there is not a single branch of the science of his day which he did not illumine and adorn — chemistry, astronomy, optics, mechanics, acoustics, mineralogy, crystallography,

botany, physiology, pathology—each in turn was the subject of his intellectual activity. It is, however, only with his chemical work that we have here to do. Reference has already been made to the part which Wollaston's paper, "On super-acid and sub-acid Salts," played in securing attention to the atomic theory; his "Synoptical scale of chemical equivalents" was of special service to the student and the manufacturer in facilitating the application of the laws of chemical combination to operative chemistry. His "Description of a reflective Goniometer" is a classic in the literature of chemical crystallography and marks an epoch in the history of that subject. His discovery of a process by which platina may be rendered malleable proved of incalculable service to practical chemistry and the arts. Some idea of what that service has been may be gained by recalling what we have secured, and secured only by our crucibles and other vessels of platinum, and by the use of platinum foil and wire, and by imagining what our position would be if we were wholly deprived of these articles. Platinum had been known for some sixty years before Wollaston's time, but as has been stated, "the very properties which made its value certain if it were wrought into vessels, forbade its being easily fashioned into them," and whole cargoes of the native metal are said to have lain unpurchased for years in London, as it could not be turned to account. Wollaston, foiled in his attempts to make a living by medical practice, became, as is well known, rich by his process of working platinum, which he carried on in secret and with the aid of a faithful old servant in his laboratory, a small detached building at the bottom of his garden in Hunter Street, Brunswick Square.

In the early part of the century the price of

manufactured platinum was less than half its present value. In 1805 platinum crucibles were to be obtained from Mr. Carey, 182 Strand, at 17s. 6d. the ounce, whilst wire was 16s. At the time of the foundation of the Chemical Society the price of manufactured platinum had more than doubled.

Wollaston first made known his process in the Bakerian lecture of 1828—the year of his death—and the Council of the Royal Society "deemed themselves bound to express their strong approbation of this interesting memoir by awarding a Royal medal to its author, and they anticipate with confidence a general approbation of what they have done." The further development of the metallurgy of platinum in this country is associated with the firm of Johnson, Matthey, and Co., who worked platinum for many years before any other commercial manufactory was established. The process of Wollaston appears to have been first worked, sometime between 1800 and 1808, by Mr. Thomas Cock, a relative of Mr. Percival Johnson, who began the refining and manufacture of platinum in Hatton Garden.

Wollaston's name is further connected with the platinum group of metals by his discovery of rhodium, which he published in 1804, immediately after the existence of iridium and osmium had been announced by Smithson Tennant, and also by his discovery of palladium. The manner in which the last-named metal was made known constitutes one of the most singular episodes in the history of scientific discovery, and affords a curious commentary on certain phases of Wollaston's character. In fact, one or two points in Wollaston's connection with the discovery and working of the platinum metals are somewhat obscure.

In 1803 appeared an anonymous circular which was sent to a number of persons, amongst them the editors of Nicholson's and Tilloch's Journals, stating that a new metal—palladium or new silver—could be purchased from Mrs. Forster, 26 Gerrard Street, Soho, in samples of five shillings, half a guinea, and one guinea each, the price being at the rate of one shilling per grain. Certain of the properties of palladium were given in the advertisement to show that it was "a new noble metal." This announcement attracted the attention of Chenevix, an Irish gentleman, well known as a Fellow of the Royal Society and for his analyses of certain minerals and for his work on oxygenised and hyperoxygenised muriatic acid. The mode adopted to make known a discovery of so much importance, without the name of any creditable person except the vendor, appeared to Mr. Chenevix unusual in science and not calculated to inspire confidence. Accordingly, with a view to detect what he conceived to be an imposition, Mr. Chenevix procured a specimen and eventually the whole quantity which had been left for sale with Mrs. Forster, who, she stated, was totally unacquainted with the person who brought the metallic substance and the printed paper to her house. Chenevix found all the statements in the paper to be correct with the exception of that of the specific gravity, which he stated to be different by one or two per cent. He made a long and elaborate investigation of the matter, and concluded that the pretended new metal was a combination of platinum and mercury. He sent in his memoir to the Royal Society, and two evenings were spent in the reading of it.

The Journal Book of the Society, the entries of which were made under the direction of the Secretaries, if not

actually by them, gives a *précis* of this paper. We read : "Thus, after having been baffled in his attempts to discover, by analysis, the component parts of this substance, which he (the author) could never bring himself to consider as a new metal, a synthetic process at length led him to the discovery that the whole pretence was an imposition, and that the substance is, in fact, a combination of platina and mercury ; in which the latter, while it masks the most characteristic properties in the former, loses the greater number of its own distinctive qualities.

"The singular fact that an alloy of two metals should be produced, the specific gravity of which is little more than one-half of what it ought to be by calculation, is, no doubt, worthy of particular attention ; and as quicksilver was in this process brought to a fixed state under circumstances never before observed, a notion might be entertained that the great desideratum in alchemy, the fixation of mercury, was by no means a visionary object. . . . Those who cultivate chemistry with any degree of ardour will be gratified to see in this paper the pains taken by the author, and the various modes he has devised, to produce this compound metal in its most perfect state of combination."

A few months later the editors of the various scientific periodicals received "under cover of the twopenny post," an unsigned paper offering a reward of £20 for the artificial production of palladium. The editor of *Nicholson's Journal* says the paper "is written in the same hand as a note which covered a small piece of palladium mentioned to have been received by me last midsummer. Upon inquiry, I find that Mrs. Forster has received the sum of £20 with instructions conformable to this paper." The paper ran as follows :—

December 16, 1803.

SIR,

As I see it said in one of your journals, that the new metal I have called palladium, is not a new noble metal, as I have said it is, but an imposition and a compound of platina and quicksilver, I hope you will do me justice in your next, and tell your readers I promise a reward of 20£ now in Mrs. Forster's hands, to any one that will make only 20 grains of real palladium, before any three gentlemen chymists you please to name, yourself one if you like.

That he may have plenty of his ingredients, let him use 20 times as much quicksilver, 20 times as much platina, and in short of anything else he pleases to use: neither he nor I can make a single grain.

Pray be careful in trying what it is he makes, for the mistake must happen by not trying it rightly.

My reason for not saying where it was found, was, that I might make some advantage of it, as I have a right to do.

If you think fit to publish this, I beg you to give the names of the umpires, as I have desired Mrs. Forster to keep the money till next midsummer, and to deliver it only in case they can assure her that the real metal is made by a certificate signed by you, and by them, on this check.

I hope a little bit of whatever is made may be left with Mrs. Forster.

Mr. Nicholson nominated Hatchett and Edward Howard to join himself as judges of the product which might be made in their presence; but the £20 was never claimed, and in 1805 Wollaston, in a memoir contributed to the *Philosophical Transactions*, announced that he was the discoverer of the new metal, to which he had given the name palladium, "from the planet which had been discovered, nearly at the same time, by Dr. Olbers."

What was Wollaston's motive in bringing his discovery to the notice of the scientific world in so extraordinary a manner can only be surmised. From the

account of his memoir given in the Journal Book of the Society, in which no mention is made of Chenevix and his work, he refers to the "concise delineation of its character" given in the advertisement of the metal, in which he says he "avoided directing the attention of chemists to the source from where it had been obtained, and thereby reserved to himself a more deliberate examination of many phenomena that yet remained unexplained in the analysis of platina, by which he was subsequently led to the discovery of rhodium." No proper explanation was given of the circumstance that he, as Secretary of the Society, allowed Chenevix's paper to appear in the *Transactions*. It must have been his duty actually to read this paper at the two meetings when it was brought before the Fellows; he must also have been responsible for the account which appears in the Journal Book, and which, as one now peruses it, in the light of what subsequently happened, is almost cutting in its sardonic humour and irony. It has been stated that Wollaston did all in his power to persuade Chenevix to withhold or withdraw his paper. Be this as it may, there is no doubt that Chenevix keenly felt the humiliation he suffered. He retired to the Continent, and all his subsequent papers were published in France.

In the year of Davy's death, Edward Turner wrote, "The æra of brilliant discovery in chemistry appears to have terminated for the present." Although the number of workers in the science was probably greater than during the first few years of the century, the volume of chemical literature actually produced, judged by the number of contributions to the various societies and to scientific periodicals, had been steadily declining during the previous ten years, and was actually not much more than half what it was in 1802. The work of the first

quarter of the century had, in fact, been that of the pioneers, and it was now necessary to secure and consolidate what had been gained rather than to push on into new territory. "The time is arrived," continues Turner, "for reviewing our stock of information, and submitting the principal facts and fundamental doctrines of the science to the severest scrutiny. The activity of chemists should now, I conceive, be especially employed, not so much in searching for new compounds or new elements, as in examining those already discovered; in ascertaining with the greatest possible care the exact ratio in which the elements of compounds are united; in correcting the erroneous statements to which inaccurate observation has given rise; and exposing the fallacy of opinions which partial experience or false facts have produced." Stoichiometrical considerations were, in fact, becoming of increasing importance. The atomic theory was now a part of the settled doctrine of chemistry. To use the words of Davy, chemistry had now passed under the dominion of the mathematical sciences. The exact numerical relations of the elements and their combinations, both by weight and by volume, Prout's hypothesis and all that it implied, were types of the questions which seemed most urgently to require solution. The idea of stating the course of chemical change with a more mathematical precision than had hitherto been possible is reflected, too, in the attempts which were made at about this period to express such changes by means of equations. Chemical symbols have been used from time immemorial, but the notion of attaching a definite numerical value to them had a rational basis only after the promulgation of the atomic theory.

Berzelius is usually credited with having been the first to suggest, in principle, the system of notation at

present employed by chemists, although there is a tradition in Glasgow that the merit should be ascribed to Dr. Thomas Thomson. Herschel, in his paper on "Hyposulphurous acid" in the *Edinburgh Philosophical Journal* for 1819, uses algebraic expressions to describe the reaction between silver nitrate and calcium thiosulphate. The idea of using a mathematical notation to express the chemical composition of substances and their mutual actions, although in common use in Sweden, France, and Germany, for a time made little progress in England, partly from the conservatism of chemists who, like Richard Phillips, failed to perceive its necessity or convenience, and to whom the language of symbols

> . . . was a Babylonish dialect
> Which learned *chemists* much affect;
> It is a party-coloured dress
> Of patch'd and piebald languages;
> 'Tis English cut on Greek and Latin,
> Like fustian heretofore on satin;

and partly from the strictures of mathematicians, chief among whom was Whewell, who condemned the Berzelian system for what he regarded as its "gross anomalies" and "disfigurements," and its want of "mathematical symmetry and consistency"—"a system which violates mathematical propriety so entirely that it must always be disagreeable to see an example of it, for any person who has acquired the first rudiments of algebra."

Whewell, whose "forte was science, and whose foible was omniscience," of course recognised that a mathematical notation might be of the greatest service in chemistry, and, indeed, could not ultimately be dispensed with, and he made a vigorous attempt "to

purify and improve the foreign system." One of the chief rocks of offence to Whewell was the method adopted by Berzelius of connecting the symbols of the elements to represent compound substances. Chemical combination is essentially an operation of addition, whereas the symbolic notation, if understood algebraically, would indicate that the constituents were multiplied by each other. There is not much force in this objection, although, as Whewell points out, we probably owe to the symbols being read algebraically the ambiguity of the words *factor* and *product*, which are sometimes used to express the ingredients producing a chemical compound by their addition, as well as the compound itself, but which in algebra properly refer to parts producing a number by multiplication. Still more objectionable to Whewell was the use of the sign +; this he characterised as "a wanton and superfluous violation of analogy."

Whewell's objections were rather more pedantic than practical. It was pointed out that the juxtaposition of symbols and the employment of an index figure, as practised by Berzelius, would lead the chemist into no error, because their subjects are not susceptible of algebraic *powers*, or of being multiplied into each other.

On general grounds, too, Whewell's system failed to commend itself to chemists, even to those who were not unmindful of the mathematical proprieties. It only approximated to mathematical consistency, and failed altogether as regards brevity and clearness. The multiplication of lines and brackets which it entailed, although algebraically just, gave a perplexing appearance to the formula, and destroyed its graphic character. The unvarying sign + alternately with every letter was also objectionable. Whewell, who was not insensible

to these defects in his system, suggested a method of contraction, but the compromise failed to realise the graphic perspicuity of the Berzelian notation, whilst, as an analytical expression, it was inferior to the extended system from which it was evolved. Whewell, moreover, avowedly put it forward as more suitable for the purposes of mineralogy than of chemistry, and the objection was naturally raised that a system which required new contrivances and contractions to suit different points of reasoning, and was so partial and so incomplete an expression of chemical composition in general, was certain to lead to interminable confusion and difficulty.

The serial chemical literature of the early thirties contains a number of interesting papers on chemical notation, not only by Whewell, but by Phillips, Prideaux, Warington, and Graham, and the merits of the various systems proposed were fairly discussed, with the result that the Berzelian method was at length universally adopted. As an example of the confusion which reigned at the time, we give, from one of Phillips's papers, the composition of common phosphate of soda as expressed in the different systems of notation then in vogue.

Berzelius	$\ddot{\mathrm{N}}\mathrm{a}\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{P}} + 24\ \mathrm{Aq}.$
"	$\dot{\mathrm{N}}\mathrm{a}^2\underline{\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{P}}} + 24\underline{\dot{\mathrm{H}}}.$
Graham	$\dot{\mathrm{N}}\mathrm{a}^2\dot{\mathrm{H}}^{24}\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{P}}.$
Rose	$\mathrm{NaO} + \mathrm{PO}^5 + 24\underline{\mathrm{HO}}.$
Whewell	$\mathrm{N} + p' + 24\ \mathrm{q}.$
Brande	$\mathrm{S} + p' + 24\ \mathrm{q}.$
Turner	$\mathrm{So} + \mathrm{P} + 2\tfrac{1}{2}\mathrm{O} + 24\ \mathrm{aq}.$
Johnstone	$\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{P}} + \dot{\mathrm{S}}\mathrm{o} + 24\dot{\mathrm{H}}.$
Prideaux	$N\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{P}} + 24\ Aq.$
Warington	$\overset{\circ\circ}{\mathrm{Po}} + \overset{\circ}{\mathrm{So}} + 24\mathrm{H}^{\circ}.$

With the exception of Graham, it cannot be said that British or Irish chemists made any very remarkable contributions to science during the decade from 1830 to 1840. The number of workers was doubtless greater than at any previous period of the century. The volume of literature, as measured by the number of papers published, was more than double that of the first decade—one of the most momentous periods of our chemical history. But the efforts of workers at the time were spent more on points of detail, on the filling in of little gaps in the chemical structure, as it then existed, than in attempts at new developments.

Thomas Clark, mainly remembered for his method of determining the hardness of water, and for his discovery of pyrophosphoric acid, was studying the metallurgy of iron. Apjohn, whose name is best known in connection with his formula for determining the dew-point from the indications of the wet bulb hygrometer, contributed a few papers on mineral chemistry; Daubeny, a professor of chemistry to botanists, and a professor of botany to chemists, occupied himself principally with geological and botanical chemistry; Colonel Yorke studied the action of air and water on lead; Everitt investigated the behaviour of dilute sulphuric acid with potassium ferrocyanide, and discovered the salt which is associated with his name; Johnston made analyses of mineral substances of organic origin, investigated paracyanogen, certain cyanides, and double haloid salts; Kane analysed a few minerals and inorganic salts, the chlorides of iodine, and studied the action of ammonia on the chlorides and oxides of mercury, the composition of essential oils, and the nature of wood spirit. Penny made a remarkably accurate series of atomic weight determinations of

chlorine, nitrogen, potassium, and sodium, which served still further to strengthen the case against the hypothesis that all equivalent numbers are simple multiples of that of hydrogen; and Gregory, the successor of Graham at the Andersonian College, a follower of Liebig, and one of the earliest chemists to introduce his methods into Britain, occupied himself with the study of pharmacological and organic products, the analysis of petroleum, and the destructive distillation of caoutchouc. Other chemical workers at this period were Pepys, Porrett, Edmund Davy, Brooke, Cooper, Daniel, John Mercer, William Herapath, Walter Crum, William West, Hennell, Scanlan, Teschemacher, Hugh L. Pattinson, Warington, Andrews, Richardson, Robert D. Thomson, Denham Smith, Golding Bird, Solly, Fownes, Stenhouse, George Wilson, and Griffin.

But the general condition of scientific chemistry with us during this period was, no doubt, accurately characterised by Liebig, when he wrote to Berzelius in 1837: "Ich bin einige Monate in England gewesen, habe ungeheuer viel gesehen und wenig gelernt. England ist nicht das Land der Wissenschaft, es existirt dorten nur ein weitgetriebener Dilettantismus, die Chemiker schämen sich Chemiker zu heissen, weil die Apotheker, welche verachtet sind, diesen Namen an sich gezogen haben. . . . Graham macht auch in wissenschaftlicher Hinsicht die schätzbarste Ausnahme, er ist ein vortrefflicher Mensch, auch Gregory, der an seine Stelle in Glasgow gekommen ist."

It was at this low ebb in our fortunes that the Chemical Society was founded. It might seem, at first sight, an unpropitious time at which to launch a new Society for the purpose of publishing the work of chemical investigators, when chemical investigation

itself was so languidly pursued. But the seventy-seven gentlemen from all parts of the kingdom who enrolled themselves as the founders of the Chemical Society appeared to have had a robust faith in its ultimate success, and in the influence it was bound to exercise in stimulating chemical inquiry in these islands.

How far their faith was well founded—how far indeed this Society has succeeded in stimulating chemical research in this kingdom, I may possibly have an opportunity of showing on some future occasion. I hope also to be able to offer some account of the development of our means of teaching chemistry in this country during the century.

XIX

ON THE DEVELOPMENT OF THE CHEMICAL ARTS DURING THE REIGN OF QUEEN VICTORIA

An Address delivered at the East London Technical College, 8th February 1897

On the 20th of this coming June (1897) all here, I trust, will be celebrating, in some way or other, the sixtieth anniversary of our Queen's accession to the throne; we shall thus be participators in an event which is unique in the history of this kingdom. Her Most Gracious Majesty's reign will be memorable not only for the number of its years, but for the fact that it is without a parallel in the history of nations in respect to the increase in material benefits and in general well-being which it has witnessed. Under the beneficent sway of our Queen this country has advanced by leaps and bounds in those arts which civilisation brings in its train; knowledge has increased; the incalculable stores of energy in nature have been drawn upon and turned to account; every branch of industry has developed, and new and undreamt of forms of it have been created. We have pushed our commerce to the uttermost parts of the earth, and our influence is felt in every region.

All this need be said in no spirit of vainglory or boasting: it is the plain and simple expression of a fact

which cannot be questioned, which every man and woman in this room knows and feels, and for which in our heart of hearts we each and all are, or should be, profoundly grateful. We know it and we feel it, in that throughout the whole of our waking existence we are constantly witness of what this sixty years of progress means to us all. The general standard of well-being and of comfort has been raised enormously; and the means to reach and maintain that standard have been multiplied a hundred-fold. What were luxuries to our grandparents are among the necessities of our daily lives: things which to them were marvels and but rarely seen, if seen at all, are the commonplaces of our existence. I do not wish to say that everything is yet of the best and for the best in the best of all possible worlds. There are two periods in a man's life when he is disposed to take, if not an optimistic, at least a cheerful view of things—when he is young and ardent, eager and sanguine; and when he is old, and inclined to rest and be thankful. I am in the unfortunate position of being neither young enough for the one, nor old enough for the other. But I may be allowed to give utterance to this conviction—a conviction which is deepened and strengthened when I seek to gain a retrospect across the span of these sixty years—that the man who is insensible to the fact that there is such a thing as an evolution of social order, making for good in the same quiet, gradual, resistless way, as in the evolution in the organic world; or who fails to perceive that the mutual play of social forces—the action and reaction amongst every class of a community blessed with free and enlightened institutions—is surely tending to the amelioration of the common lot by the operation of laws as certain in their effect as the law of gravitation itself:

that man is very short-sighted or very ignorant—ignorant of the true lessons which sociology teaches—or incapable of perceiving their real significance.

The old Roman proverb which bids us to hasten slowly is, in matters relating to the social organism, the expression of a profound historical truth. The material fabric is not the less durable because the cementing union between its various sections has taken longest to consolidate. The disintegrating influence of the ages is most surely resisted by those structures which have taken most time to put together, and in which most time and most pains have been spent in adjusting the several parts.

It seems to me, then, particularly fitting that here in the Queen's Hall, we should on the eve, as it were, of the Queen's Diamond Jubilee, as it has come to be called, seek to trace, it can only be in the broadest possible outline, some of the more significant features in the growth in intellectual and material welfare of Her Majesty's subjects during the threescore years that she has controlled the destinies of her mighty empire.

Believe me, it is a lofty and inspiriting theme, but all too vast to be dealt with by one poor brain. I am therefore constrained to follow the advice of the old proverb which says that the shoemaker should not go beyond his last, and to confine myself to those matters concerning which it may be thought that by experience, reflection, and reading I may have some little claim to venture upon an opinion.

It has been said that you may gauge the civilisation of a country by the amount that it spends on soap, and it hardly requires a moment's thought to see the significance and truth of that remark. Indeed, as I walk along the streets, or ride in omnibuses, or look out of the windows of railway carriages, I am constantly

reminded of the saying, and as my eye lights upon a succession of alluring but obtrusive posters, not always in the best style of decorative art, I console myself with the reflection that after all they are so many appeals to the highest instincts of our nature. I do not, however, now wish to speak of soap in its ethical and moral aspects. For the moment, I take much lower ground. Soap is a manufactured article in the production of which many industries are concerned, and, therefore, if soap is an index of civilisation, the prosperity of the industries concerned in the production is also the outward and visible sign of a civilising agency. Now the characteristic of these industries is that they are in the main chemical. I am not conscious of any fault in my logic, even on the restricted basis from which I started, if I claim that the position of the chemical arts in a country is a measure of the material prosperity and civilising power of that country. It is not necessary for me to labour this point. Chemistry and its applications concern us at every turn. There is hardly a single industrial operation which could be named with which this science has not some relations, either proximately or remotely. Whether we are rich or poor, industrious or idle, in health or in sickness, we are beholden to chemistry and the chemical arts for a thousand beneficent things. People nowadays have very short political memories, but I daresay there are some in this room who can still recall that oracular utterance in which the late Lord Beaconsfield, from his place of power in Parliament, congratulated the country on the prosperity of its trade in chemicals. A good deal of cheap and harmless chaff was levelled at that statesman in consequence, all of which I have no doubt he took with the serene indifference and philosophic calm that characterised him. And

I venture to think you will agree with me he had good reason for so doing, as there was more wisdom in the remark than the critics of the hour choose to give him credit for. Lord Beaconsfield had a sounder view of what constituted an index of a nation's prosperity than those who sought to make merry at his expense.

Now it is of the material progress of this country, as manifested by the difference in the position of chemistry and of the chemical arts at the period of the Queen's accession and in their position at present, that I wish to dwell for a few moments. The time at my disposal is far too short to enable me to do more than indicate, as it were, in a few touches and in the very broadest outline, the main points of contrast. Let us throw our mental eye backwards and try and realise the state of things in chemistry, pure and applied, at the time of the Queen's coming to the throne. What was the condition of chemical science in Great Britain in 1837? Who were engaged in discovering its truths, and in promulgating its doctrine? And how were the facts of the doctrine being made subservient to the welfare and material interests of the nation? To ask a man who is abreast of the chemistry of to-day—and if he is abreast he is also in all probability ahead; that is, he is seeking to push his way beyond the frontiers of contemporary knowledge,—to ask such a man to look at chemistry from the standpoint of sixty years ago, and to try and form to himself a mental picture of its condition at that period, is really like asking the historian to jump back into prehistoric times. Chemistry, as an art, has been practised, of course, from time immemorial, and a great variety of chemical products—metals, salts, acids, dyes, pigments, were made long before the Christian era, although I must confess that soap is a comparatively

late invention, a fact which, it may be argued, serves to explain the peculiar and restricted character of the ancient forms of civilisation. Chemistry as a science, however, is barely a century old. It is based upon the atomic theory, and the idea of explaining chemical phenomena by the conception of atoms was first clearly and definitely stated by John Dalton during the first decade of this century. The whole course of chemistry, however complex and many-sided it may seem, is really one vast elaboration of the atomic theory. As Liebig has said: "All our ideas are so interwoven with Dalton's theory, that we cannot carry ourselves back to the times in which that theory did not exist." And yet this fundamental hypothesis, as understood by chemists, had barely come of age when the Queen came to the throne; it was not much older at the time than she herself. The illustrious philosopher who gave this conception to the world was still living, but stricken down, I regret to add, with the paralytic attack, the beginning of the brain disintegration, which seven years later ended in his death. The great Sir Humphry Davy, a younger man than Dalton, and the pioneer in the vast field of electro-chemistry which to-day is yielding such rich treasure, had only been dead about eight years, and Michael Faraday, of revered memory, blacksmith's son, newspaper boy and bookbinder's apprentice, reigned in his stead at the Royal Institution. The Fullerian Professor of Chemistry, as he had come to be designated, was then in his forty-fifth year, in the full maturity of his intellectual power, and near the meridian of his scientific fame. But all his work in pure chemistry was practically done, and he was almost wholly engaged upon those great problems of electrical science which have made the extraordinary

development of applied electricity as we see it to-day in electrolytic decompositions, in the electric light, and in the applications of electricity as a source of power, alone possible. The Queen, indeed, may be said to have witnessed as Queen the birth of this marvellous application of natural energy, to have lived with it through its vigorous youth, and to have seen the promise of a fruition so vast that no man can set bounds to it. She might have seen the simple, fundamental, and yet ever-striking experiments out of which has grown the multifarious machinery that modern industrial electricity has called into existence. Try to realise the difference between Faraday's simple home-made apparatus, his copper discs, his bits of soft iron, wound with wire, insulated with calico and twine, and the mighty dynamos which are converting the energy of "a harnessed Niagara" into heat and light, and chemical action, and supplying power to a continent!

And all this within the span of a single reign; within the compass of a couple of generations! This astonishing movement is what historians will ever recognise as the characteristic feature of the Victorian Era. It has wholly changed the economic and social condition not only of our own people, but of every country which has had the intelligence and the wisdom to participate in it, or to avail itself of its fruits. It has acted and reacted not only on industry, and on the material benefits that attend on industry, but on every department of intellectual power. It has changed, although hardly with a commensurate rapidity, the face of our educational system. To judge what the change has been, let us try to realise how chemistry was taught in 1837. As a part of school education it was practically unknown, although children whose parents had the good fortune

to be influenced by the teaching of such far-sighted men as Mr. Edgeworth, had their curiosity stimulated and fed by occasional lectures on science.

As regards the Universities, at Oxford there was Dr. Daubeny, a prolific writer on volcanoes, and on rural economy. At Cambridge there was Professor Cumming, who, so far as I know, has not enriched the literature of chemistry proper with a single contribution. At neither place was there anything in the nature of a laboratory which the student could attend. If the enterprising undergraduate desired to familiarise himself with the facts of chemistry by practical experiment, or to try and work out an idea which might have occurred to him, he had to pursue his inquiries in his own rooms, and with such apparatus as his means or his opportunities could command, to the imminent risk of his furniture, and to the dismay and disgust of his bedmaker. It was under such conditions that the late Sir John Herschel discovered the solvent action of what the photographer calls "hypo," and thereby made the photographic calling possible.

In Scotland, Dr. Hope, whose name carries us back into the preceding century, still enjoyed at Edinburgh the fame as a lecturer which he shared with Davy at the Royal Institution, but no tuition in practical chemistry was ever thought of. Matters at Glasgow were a little better, and Thomas Thomson would occasionally extend a brusque hospitality to the student who aspired to learn the art and mystery of mineral analysis, but no systematic instruction was ever attempted. The youth with no knowledge of manipulative work, and with scarcely an acquaintance with the forms even of chemical apparatus, might be set at the

very outset to struggle with a zeolite or grapple with an atomic weight determination as best he might. In London there was the promise of better things. Thomas Graham, who had already made his memorable discovery of the law of gaseous diffusion, whilst Professor of Chemistry at Anderson's College in Glasgow, had followed Ure and Birkbeck to London, and had been elected to the Chair of Chemistry at University College, Gower Street, up to then known as the University of London. Here he created the School of Chemistry which, aided by Fownes and Williamson, he made famous throughout Europe. But it may be doubted whether in 1837 there were more than a couple of dozen persons in the British Isles altogether receiving systematic instruction in practical chemistry, and even that supply was probably fully equal to the demand. There was, in fact, little to tempt men to take up chemistry as a means of livelihood. Teacherships were few in number; analytical chemistry as a profession hardly existed, and chemical manufacturing was done by rule of thumb, and for the most part very badly done. I have heard chemical manufacturers of two generations ago tell very strange stories of the rough-and-ready manner in which many of their operations were conducted. A vitriol plant, for example, of sixty years ago was a very different thing in point of efficiency from what it now is, and such things as condensing towers in the Leblanc process of converting salt into soda-ash were almost unknown. All the muriatic acid was freely sent into the air, and Government Inspectors never troubled the peace of mind of the manufacturer. If you turn over the pages of the first edition of Ure's *Dictionary*, or look through the early reports of the British Association —all of them works within your own reach—and com-

pare the accounts with the specialised treatises—some of them almost monumental works, of which the last decade has been prolific—you will gain a vivid conception of the vastness of the difference in the character, variety, and volume of the work at the extremes of these sixty years.

How the state of things appeared to a well-informed and intelligent observer is well illustrated by one of Liebig's letters to Berzelius, in which he recounts his impressions of England, which he had just visited. Under date 26th November 1837, he tells the Swedish chemist that he had been some months in England, had seen a vast amount, and learnt little. England, he says, is not the land of science; our chemists were ashamed to call themselves chemists because the apothecaries had appropriated the name. He was extraordinarily pleased with us as a people, and delighted with our hospitality and welcome, but as regards our chemists, Graham was the most precious exception. "He is an excellent fellow, as also is Gregory, who has succeeded him in Glasgow."

But a little leaven was leavening the whole lump, and the leaven was Liebig himself. Aided by the far-sighted munificence of the Grand Duke of Hesse-Darmstadt, he had established the little Giessen Laboratory, and thither every aspirant for chemical fame bent his steps. Among the earliest of these chemical pilgrims were Lord Playfair, Sir Henry Gilbert, Professor Williamson, Dr. Gladstone, and others who might be named, some, happily, still amongst us, but many of whom have now passed over to the majority. All these men brought back with them something of the spirit and of the method which have made the little laboratory on the banks of the

Lahn famous in the history of chemistry. This influence has been like the seed which fell on good ground; it has sprung up and multiplied an hundred-fold. It has made Germany pre-eminent in the world of scientific chemistry, and it has had a profound effect on chemical activity and on the development of chemical teaching in this country.

One of its first results was seen in the creation in 1841 of the Chemical Society—a society whose function and privilege it has been to foster by every means in its power the spread of chemical knowledge and chemical inquiry in this country. How it has achieved its purpose may best be seen in the roll of its membership, in the vitality and activity of its meetings, in the long succession and value of its publications, and last, but not least, by the helping hand it extends to the young investigator, by the wise and prudent administration of the funds which have been placed at its disposal by private benefactors and public bodies, among whom I gratefully recall the Drapers' Company. And here I may be permitted to say that among all the benefactions which the Company has scattered broadcast with so generous a hand, there has been none better applied, or been more fruitful in result, than that by which they have thus fostered chemical research. I claim to say this because it is my duty and privilege as the Treasurer of the Chemical Society to aid in the administration of this fund, and to take note of, as a member of the Committee of Publication, the memoirs and papers which follow from its operation.

Another notable result of Liebig's influence, and of the awakening which resulted from it in this country, was the foundation in 1845 of the Royal College of Chemistry, to which Hofmann, one of the favourite and

most distinguished pupils of Liebig, was invited—thanks to the action of the late Prince Consort. What Hofmann, fired by the example of Liebig and his own innate enthusiasm, did for chemistry in England time will not allow me to dwell as I should like to do. To the chemical students of this place I would say there is no more inspiriting or instructive chapter in the history of chemistry in this country than that which records the work of the Royal College of Chemistry as set forth in that tripartite panegyric of Hofmann—the joint work of Lord Playfair, Sir Frederick Abel, and Professor Armstrong—which you will find in the *Journal of the Chemical Society*—a publication which doubtless is to be met with on your library shelves. What the Royal College of Chemistry was to British science there are happily many living witnesses to testify. We have still amongst us Sir F. Abel, Dr. Perkin, Mr. Crookes, and last, but not least, Mr. Heywood. Others have passed away, among them Warren de la Rue, Mansfield, How, Medlock, Rowney, Field, Nicholson, Bloxam, Noad,—all of whom have left their mark on the development of pure or applied science in this country. But if Hofmann has left us, his spirit and his example still remain and actuate us. This spirit is being carried into a hundred places of chemical instruction and research in these islands to-day. Let us pray that it may continue and increase, for it is on its continuance and growth that the progress of chemical science and chemical industry depends, and in so far as our national prosperity is connected with the progress of the chemical arts, our national prosperity depends on it also.

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